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FIELD AND LABORATORY METHODS IN THE STUDY OF METAMORPHIC ROCKS*

By MARLAND P. BILLINGS†

The successful study of metamorphic rocks requires a skillful integration of field, office, and laboratory work. The laboratory investigations include descriptive petrography, chemical analyses, and the laboratory phases of petrofabric analysis. Special but important facets of the problem include the physical chemistry of silicate systems and studies of the solid state. The office work involves the analysis and correlation of data obtained in the field and laboratory. The field work includes the gathering and integration of a vast amount of data obtained in thousands of isolated exposures. It has been said that field geology consists of the collection and analysis of significant facts, but this is equally true of laboratory and office work. Which facts are significant depends, however, on what we are trying to do. Facts that pass unnoticed or are considered unimportant for many years, moreover, suddenly take on new significance. Nevertheless, long experience in any field of human endeavor shows that the best results can be accomplished by certain methods.

Our one great objective in the study of metamorphic rocks is to learn the nature of the processes that produced them. Although this is a tremendous problem, the task is somewhat simplified, because, as has long been recognized, four principal processes are involved: (1) sedimentation, (2) deformation, (3) metamorphism, and (4) plutonic activity. To discuss the sedimentation, however, it is not only necessary to know the stratigraphy of the area with which the investigation is especially concerned, but also the stratigraphy of surrounding regions and the paleogeographical environment. To discuss the mechanics of deformation, it is essential to know the geological structure, not only of the local area, but also of the adjacent regions. To discuss the metamorphic processes it is essential to study the petrography of the metamorphic rocks, their chemical composition, and their fabric. To discuss plutonic processes in relation to metamorphism it is necessary to study the petrography, chemistry, and spatial relations of the plutonic rocks.

The objective of the present paper is twofold. One purpose is to try to decide which facts are significant. A second aim is to determine the significance of these facts. The two questions are obviously closely related.

Which facts, in the present state of knowledge and in the time available to complete the investigation, are most likely to lead to a solution of our problems? We would all agree that the grouping of the rocks into mappable units, the measuring of the attitude of bedding and cleavage, and the recording of the attitude of the axes of folds all constitute significant data. On the other hand, how important is it to measure the attitude of the axial planes of folds, the attitude of joints, or the orientation of the c-axes of quartz? Conservation of time is essential in all scientific investigations.

The interpretation of the facts is even more controversial. For example, what is the significance of lineation or boudinage? Have the beds been thrown into undulations by flexure-folding or shear-folding? How are the minor folds related to the major structure?

It will be necessary to confine the discussion to half a dozen major topics because of the limitations of space. An attempt will be made to decide which facts are

* This paper, illustrated with lantern slides, was presented at the meeting of the Section on November 6, 1950.

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significant and, especially, what those facts indicate regarding geological processes.

Stratigraphy of Metamorphic Rocks. In a recent paper (Billings, 1950), the author has emphasized that students of metamorphic rocks should be well trained in stratigraphy, not only to solve the immediate problems with which they are concerned, but also to understand the paleogeographic setting of their rocks. Further discussion here is unnecessary.

Metamorphic Zones or Facies. It has long been recognized that, under different conditions of metamorphism, a body of given chemical composition may appear in several different mineralogical assemblages. A well-known example is shale, which may become slate, phyllite, schist, or gneiss, depending upon the grade of metamorphism. Another example is basalt, which may become green schist, epidote-albite amphibolite, or amphibolite. Consequently, students of metamorphism should indicate the grade of metamorphism on maps. At present, however, three principal classifications are in use, a fact that is at least annoying to metamorphic geologists but completely baffling to stratigraphers and general geologists. The three classifications are not as incompatible as they may first appear, because all recognize the same broad grouping of metamorphic rocks. Grubenmann and Niggli (1924, p. 375) recognized that the regionally metamorphosed rocks could be classified into three major groups on the basis of mineralogy and texture. They originally believed that the variations were due primarily to differences in the depth of burial and assigned the names epizone, mesozone, and katazone, to the shallow, intermediate, and deep zones, respectively. The main objection to this system was the assumption that depth of burial is the chief cause of the zoning. Because the system treats pressure, temperature, and stress as three dependent variables, moreover, it theoretically does not have sufficient flexibility. Nevertheless, the Grubenmann-Niggli system shows very accurately the rocks found in different zones in areas of progressive regional metamorphism. If one considers the zones as temperature zones rather than depth zones, the classification is fairly objective.

Eskola (1920) introduced the facies classification, which arranges metamorphic rocks into ten or more major groups, based on depth and temperature. In his recent excellent treatise, Turner (1948), although

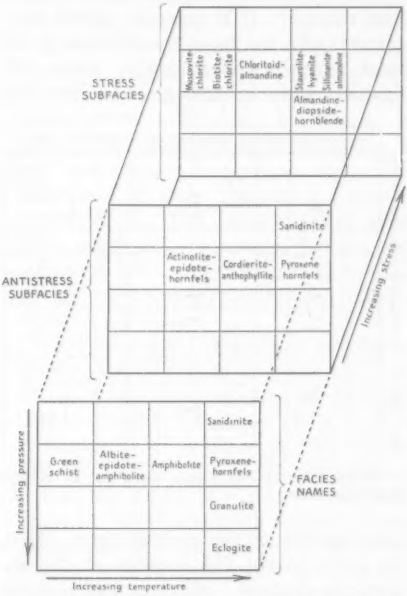

FIGURE 1. Facies classification with three variables: pressure, temperature, and stress.

using the facies classification, also accepted Harker's concept that stress is another variable. Turner's discussion indicates that he has three variables, temperature, pressure, and stress, as shown diagrammatically in FIGURE 1. In effect, he has followed Eskola in establishing the main facies on the basis of temperature-pressure relations, but has established subfacies on the basis of the presence or absence of stress.

A third classification, strictly applicable only to areas of regional metamorphism, is

that initiated by Barrow over fifty years ago (Turner, 1948, p. 35). Barrow noted that as one approached areas of progressively higher metamorphism, new minerals appeared in orderly succession. In argillaceous rocks, biotite, garnet, staurolite, and sillimanite appear progressively as one approaches the area of higher metamorphism. The line where each new mineral appears is known as an isograd.

Which system should the harrassed field man employ? It is generally agreed that, theoretically, the facies classification is the most logical. Unfortunately, many difficulties arise in trying to utilize this classification at the present time. First, we come to the old question of the importance of stress. Many geologists doubt that stress plays any important role. Secondly, Turner rejects a number of facies as invalid, which suggests that this classification is difficult to put into practice. A third problem involves the choice of rocks to be used as a basis of the classification. Turner, following Eskola, bases the main facies on rocks of basaltic composition, but the subfacies are based on rocks of different composition, in many instances, argillaceous rocks. It is possible to fit two contrasting isochemical series, such as basalt and shale, into one system, if the changes in mineralogy always occur at the same temperature and pressure in the two different rocks. This, however, is exceedingly unlikely. The point can be best illustrated by FIGURE 2, in which the main facies, represented by one type of rock, such as basalt, are shown by solid lines and capital letters. The subfacies, based on a more sensitive rock of different composition, such as a shale, are shown by dotted lines and dotted numbers. Because of extensive overlaps, the two contrasting isochemical series cannot be readily fitted into a single classification.

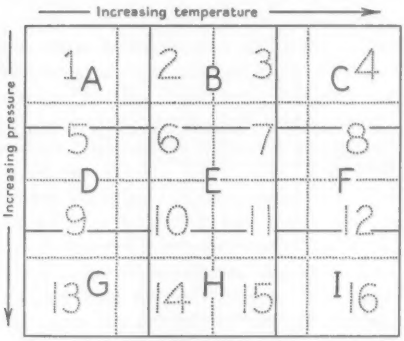

FIGURE 2. Facies classification based on two different isochemical series.

A fourth difficulty involves the application of the facies classification in detail. Turner (1948, p. 85) gives two equations for the formation of sillimanite. He assumes that the two reactions take place at the same TP relations, but *a priori* reasoning suggests that it is exceedingly unlikely that this is true. Moreover, Heald (1950, p. 74) has shown in the Lovewell Mountain quadrangle of New Hampshire that muscovite and sillimanite can exist together in equilibrium, although Turner assumes that is not possible.

Finally, the supposed high pressure facies are in considerable disrepute. Turner himself believes that the so-called glaucophane schist facies is not a high-pressure facies and many geologists doubt that eclogite indicates high-confining pressure.

It appears, therefore, that none of the three classifications in use at the present time is entirely satisfactory. It is probably true that the facies classification has the greatest potentialities. But it will undoubtedly undergo much revision, and considerable confusion will exist for many years. In areas of regional metamorphism, the isograd system is the most practical.

Minor Folds. Although most investigators of metamorphic rocks pay some attention to minor folds and make some effort to record and interpret them, far more work along these lines should be done. The attitude of the fold axis and the attitude of the axial plane should be recorded (Billings, 1942, p. 34–35). It is also important to record the pattern of the folds as shown in plan. They may show sinistral (left-handed), dextral (right-handed), or neutral patterns (White and

Jahns, 1950, p. 197). The attitude of the long limbs of the folds should also be recorded; for a neutral pattern, it is essential to record the trend and the average dip (White and Jahns, 1950, p. 196).

The critical reader may well ask whether the systematic recording of such data is worth the time and especially whether any valuable conclusions can be drawn therefrom. Are these significant data or are we boondoggling? All this information is necessary to ascertain the structural fabric of the area and it is impossible to prepare correct cross sections without such information. Moreover, when used with discretion and properly correlated with other data, valuable deductions may be made about the larger structures and the tectonic forces involved.

It is quite true that, even in single outcrops, the minor folds may show a baffling variation in the fold pattern, in the attitude of the axial planes, and in the attitude of the fold axes. But experience in New England shows that in general a systematic arrangement of the minor folds may be readily recognized. Not only are the folds in single outcrops relatively systematic, but also large areas show a distinctive fabric. As examples of systematic arrangements, we may cite the Mt. Washington area (Billings, 1941, Pl. 10) or the Woodsville quadrangle (White and Billings, 1951). Recent field work in central Vermont shows, moreover, that the minor folds are remarkably systematic.

Information on the attitude of minor folds cannot necessarily be used to deduce the major structure. If correlated with other information, however, the minor folds may be exceedingly helpful in deciphering the major structure. Most geologists are familiar with the principle of the use of drag folds to determine the major structure (Billings, 1942, p. 76–82).

Unfortunately, many minor folds do not bear this simple relation to the major structure. Hills (1940, p. 90) has suggested describing the well-behaved minor folds as congruous and the badly-behaved ones as incongruous. The incongruous minor folds may be assigned to several categories. Some found in parts of Vermont have been called flowage folds by Bain (1931); they are the geometrical opposites of drag folds. A second group of incongruous minor folds, also found in Vermont and New Hampshire, is the result of two stages of deformation (White and Jahns, 1950, p. 198–200; Moore, 1949, p. 1646–1647). Closed isoclinal folds that developed during an earlier stage of deformation have been refolded during a second stage.

Some of my colleagues working in eastern Vermont suggest that a third category of minor folds may develop when a dome of highly plastic sedimentary rocks rises. In such a rising dome, the interior would move past the exterior to produce minor folds that have a sense opposite to that of normal drags. Still other types of minor folds, which form in a rising dome of rock salt, have been described recently by Balk (1949). The axes plunge outward from the center of the dome.

Flexure Folds and Shear Folds. In flexure folding, the individual beds are bent and the beds slip past one another (Billings, 1942, p. 87–89). The crust of the earth is shortened in a direction parallel to the direction in which the compressive force is acting; the length of a bed is essentially the same before and after folding. Shear folding (Billings, 1942, p. 90–91) can be the result of small displacements along closely spaced fractures. The length of a bed, measured in cross section, is much greater after folding than before. The fractures along which the displacements take place can be so closely spaced that they are megascopically invisible, or actual planes may not exist.

Cloos (1947), in his recent superb study of oolite deformation in the central Appalachians, concluded that the folds are shear folds. If this is correct, it is of the utmost importance to students of metamorphic rocks. Associated with these folds are deformed oolites, presumed to be originally spherical, which are now ellipsoidal. In general, the intermediate axes of these oolites are parallel to the fold axes, the long

axes lie in the axial-plane cleavage and perpendicular to the fold axes, and the short axis is perpendicular to the axial plane cleavage.

Cloos's explanation involves a combination of shear folding plus flowage. That is, the original spheres are deformed into ellipsoids by two processes acting simultaneously: (1) minute displacements along the cleavage planes, and (2) shortening at right angles to the cleavage causing elongation in the direction of the long axes of the oolites.

Shear folding alone could not produce the observed results for two reasons. At the nose of the folds, the displacement along the foliation plane must reverse itself, that is, below the nose of an overturned anticline, the relative movement along the slip

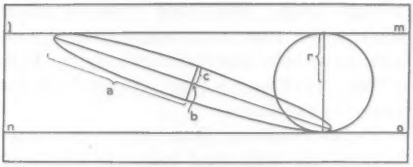

FIGURE 3. Sphere (circle) deformed into ellipsoid (ellipse) by shearing along planes parallel to *lm* and *no*.

planes is a thrusting or forward movement, but the movement is a normal or backward movement above the nose. This reversal of movement is difficult to explain mechanically. There is no relative movement at the nose of the folds, moreover, and any spherical body would still be spherical after the deformation.

The most compelling argument, however, is the fact that the long and intermediate axes of the oolites are parallel to the cleavage of the rocks. As shown in FIGURE 3, if a sphere were deformed by shear folding into an ellipsoid with 200 per cent elongation—that is, the long semi-axis of the ellipsoid is three times as long as the radius of the original sphere—the *ab* plane of the oolite makes an angle of about 18° with the slip planes. Actually, Cloos found the maximum elongation that he could measure to be less than 200 per cent, and generally it was much less. The less the elongation, the greater the angle between the *ab* plane of the oolite and the cleavage. To combine shear folding and flowage due to squeezing at right angles to the cleavage does not help. There would still be an appreciable angle between the cleavage and the *ab* plane of the oolites.

It seems to the present author that there is a more satisfactory way to explain the deformed oolites and associated folds in Maryland. When stratified rocks are subjected to horizontal compression, they are deformed by flexure folding into anticlines and synclines with vertical axial planes and horizontal axes. With further compression, the rocks begin to flow upward and each particle is elongated vertically and shortened at right angles to the compression. Deformation parallel to the fold axes is slight. A flow cleavage develops at right angles to the compression. Thus the *a* and *b* axes of the oolites lie parallel to the cleavage, a relationship impossible to explain by any hypothesis involving shear folding. In Maryland, of course, the sediments were caught in a gigantic couple, so that the axial planes of the folds and the cleavage dip southeast.

Lineation. In recent years, there has been renewed emphasis on lineation (Knopf and Ingerson, 1938; Billings, 1942; Cloos, 1946). The interpretation of the lineation is a complex problem and different kinds of lineation have different significance (Billings, 1942, p. 234–239). One of the most fascinating types is displayed by "stretched" pebbles and oolites. In many cases, it is clear that these ellipsoidal bodies were originally more or less spherical. Everyone agrees that the rock mass has been elongated parallel to the long axis of the pebble, shortened parallel to the short axis, and unchanged, elongated, or shortened parallel to the intermediate axis. There has been considerable disagreement, however, concerning the attitude of the long axis of the pebbles relative to the fold axes. There has been even more argument on the mechanism of the elongation where there has been agreement on the facts.

In 1942, the author (Billings, 1942, p. 234–236) discussed only those examples in which the long axis of the pebble lies in the bedding plane and is parallel to the fold axis (FIGURE 4C). Cloos (1947a) later pointed out, quite rightly, that another very common fabric, known for 100 years, is that in which the long axis lies in an axial plane cleavage and perpendicular to the fold axes (FIGURE 4B). A third type of fabric is that in which the long and intermediate axes of the pebbles are parallel to an axial plane cleavage, with the long axis parallel to the fold axes (FIGURE 4D). This

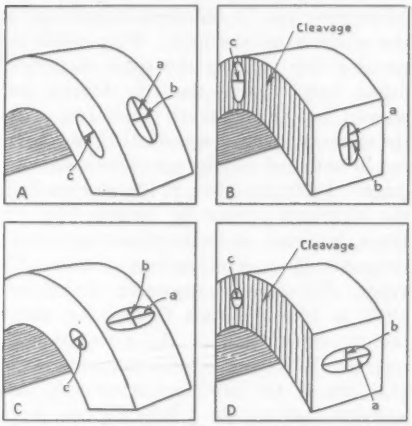

FIGURE 4. Elongated pebbles.

type occurs at Newport, Rhode Island. A fourth type of fabric is that in which the long and intermediate axes of the pebbles lie in the bedding plane, with the long axis at right angles to the fold axes (FIGURE 4A). Such an orientation has been observed by J. B. Thompson in central Vermont.

It is obvious that explanations must be found for all these types of lineation shown by elongated pebbles. Stretched oolites that lie in an axial plane cleavage and are elongated perpendicular to the fold axes have been discussed on a previous page.

For those cases in which the long axes are parallel to the fold axes, the author believes that the explanation offered earlier is correct (Billings, 1942, p. 234–236). The essence of this hypothesis is that the entire rock mass is elongated vertically by folding, as a result of horizontal compression. Horizontal elongation at right angles to the compressive force is accomplished by "stretching" of the beds parallel to the fold axes. Where the long and intermediate axes of the pebbles lie parallel to the bedding on the limbs of non-isoclinal folds and the long axis is perpendicular to the fold axis, it is apparent that the limb of the fold has been stretched.

Boudinage. Boudinage, or "sausage structure," has been familiar by that name to American geologists for at least 20 years, but comparatively few field data have been collected. Cloos (1947b) has

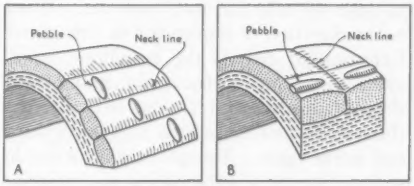

FIGURE 5. Boudinage.

recently summarized the literature. As a result of stretching, the beds "neck" or actually break. The "neck-line" is the line along which the maximum thinning or breaking takes place (FIGURE 5).

Dr. James B. Thompson and Mr. Alexander Jones have demonstrated to the present author the complexity of boudinage in Vermont and British Columbia. It is apparent that the neck-line is perpendicular to the stretching of the rocks. Dr. Thompson informs me that this conclusion is corroborated in central Vermont by the relation between the neck-line and the long axes of deformed pebbles. Where the deformed pebbles lie in the bedding and the long axes are perpendicular to the fold axes, the neck-line is parallel to the fold axes (FIGURE 5A). But where the deformed pebbles lie in the bedding plane and the long axes are parallel to the fold axes, however, the neck-line is perpendicular to the fold axes (FIGURE 5B).

Direction of Movement, or Tectonic Transport. The concept of the direction of movement, or tectonic transport, arose in the Alps, where the deformation may be compared with the sliding of cards over one another. The direction of movement or tectonic transport is toward the north in the Alps. If we could believe that all folding is shear folding, this concept would be useful in folded rocks. But if one is so conservative that he still believes in flexure folding, how useful is the concept of the direction of tectonic transport?

Let us imagine folds produced in a pressure box by a piston moving from right to left. A point on the top of an anticline has moved upward and to the left. A point at the left-hand base of an anticline has moved only forward, however. What is the direction of movement in this mass? Referring to any of the anticlines, moreover, the beds on the right limbs have sheared upward and to the left, whereas those on the left limbs have sheared upward and to the right. The movement picture in such a rock mass is complex in the extreme.

The terminology in such studies is unsatisfactory. Cloos (1946, p. 5) has criticized the usage of Gilully, Ingerson, Knopf, and Billings, who defined *b* as the lineation. Sander's original terminology is admittedly ambiguous (Cloos, 1946, p. 5). One can find support for defining *b* as either the lineation or the fold axes.

Cloos advocates defining *b* as parallel to the fold axes, *a* as parallel to the direction of movement, and *c* as perpendicular to the *ab* plane. The definition of *b* is based on what is seen, *a* is based on inference. This practice is not commendable. Moreover, what do we do if the stretching is parallel to the *b* axis? The terms *b* and *a* are then one and the same. Cloos (1946, p. 26–28) also abandoned his own definition that *b* is parallel to the fold axes, for he spends several pages discussing fold axes parallel to *a*. Therefore, he is really defining *a*, *b*, and *c* in terms of the movement.

A dual nomenclature would probably be most useful, one for what we see, the other for what we infer. To develop this thesis here seems undesirable, however, because the concept of a single regional movement direction is not very useful, except, perhaps, in areas of thrusting.

Structural Petrology or Petrofabrics. Ingerson (1944) says that structural petrology or petrofabrics "is nothing more than very detailed structural geology or petrology." It differs, however, from other methods of analysis in that it involves very detailed and extensive studies of the rock fabric by the universal stage. It is certainly time-consuming. The data obtained, moreover, can generally be interpreted with only moderate success. In many cases, the information can be obtained by an inspection of the hand specimen. Why bother to spend a day plotting the poles of biotite flakes, only to find that the biotite lies parallel to the foliation? It is true that the orientation of the space-lattice of quartz can be obtained only by use of the universal stage. Fairbairn (1949, p. 10) has classified the diagrams shown by quartz into 12 types, however, and recognizes that many diagrams show combinations of these 12 types. We discover, moreover, (Fairbairn, 1949, p. 117–133) that there is no satisfactory explanation of the orientation of quartz. Why waste time gathering data that are of no direct application to the problem before us? Why spend four months studying a stream boulder from the Alps to discover that there are five *S* planes in the rock, two of which are "statistical"?

By these remarks I do not mean that no significant data have been obtained by studying the orientation of the space-lattice by the universal stage. Ingerson (1944) cites some of the contributions of structural petrology. Fairbairn (1949) discusses clearly and judiciously what can be done by this method. It is obvious that studies by the universal stage will be important in investigating rock deformation. But this will be true only when more experimental data are available.

In trying to advise students undertaking the investigation of a metamorphic terrain, we need only quote Fairbairn (1949, p. 181): "In most examples the available petrofabric

facts far exceed the interpretation possible at the present time and to some extent, therefore, the time consumed in determining regional mineral orientation is not commensurate with the results obtained." In principle, everyone agrees that any petrofabric study in the laboratory should be merely a part of a careful and complete study. Unfortunately, many students make the petrofabric analysis without adequate field investigations. In particular, their studies are unsatisfactory, because the stratigraphy has been treated too casually. In many studies in recent years, therefore, the emphasis has been on the universal stage instead of the feet.

Experimental Geology. It is apparent that a great deal of experimental work can be done in metamorphic geology. The study of silicate systems in the laboratory has been of inestimable aid in investigating the processes that lead to the formation of igneous rocks. The laboratory study of silicate systems under conditions comparable to those that lead to the formation of metamorphic rocks has just begun. The development of new, ingenious apparatus that permits the attainment of both high temperatures and high pressures is most encouraging. Such studies, although fraught with difficulties, will greatly enhance our knowledge of the metamorphic processes. Similarly, our knowledge of space-lattice orientation will be greatly increased by controlled laboratory experiments. More emphasis should be given to solution and redeposition as well as the mechanical processes, however. It is also obvious that the study of rock deformation under high confining pressures and high temperature is important. It is rarely possible for an individual to have the time to devote to both extensive field work and detailed laboratory experiments. It is inevitable that some geologists will be primarily field geologists, others laboratory investigators. But the laboratory geologist should be familiar with field geology, and the field geologist should understand the methods and results of the laboratory experiments.

Summary. The study of metamorphic rocks is a colossal task. There are many facets to the problem and it is inevitable that the contributions of different investigators will be along different lines. By its very nature, much scientific research leads up blind alleys. But a field geologist should fully understand his objectives and should utilize his precious time in the most economical way possible. In recent years, there has been a strong tendency to go off on tangents, to neglect important phases of the problem, and to glamorize unprofitable lines of attack.

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SECTION OF PHYSICS AND CHEMISTRY

NEW USES OF X-RAYS AND OF ELECTRON DIFFRACTION*

By J. J. TRILLAT†

In this paper, it is proposed to give a brief rapid description of some new methods lately developed at the X-ray Laboratory of the National Center of Scientific Research. These may be divided into three classes, as follows: (1) methods involving use of photoelectrons (electronic radiography); (2) methods involving diffraction of X rays (study of surface structure); and (3) methods involving diffraction of electrons (study of fresh metallic surfaces and of the properties of these).

I. Methods Involving Use of Photoelectrons: Electronic Radiography

These methods fall into two groups: (a) radiography by reflection; and (b) radiography by transmission.

(A) *Electronic Radiography by Reflection.* The principle of the reflection-method, described as early as 1940, is as follows:

Consider a flat surface of any size, containing elements having appreciably different atomic numbers. When irradiated by X rays of suitable wave-length, it becomes the source of secondary waves originating by scattering and by fluorescence. It also becomes the source of photoelectrons of kinetic energy given by the well-known formula:

$$(\tfrac{1}{2})mv^2 = h\nu - W_n$$

wherein ν stands for the frequency of the X rays and W_n for the work required to extract an electron from the level n. For a given n, W_n depends on the atomic number of the atom in question.

The number of photoelectrons depends on both the wave-length and the intensity of the incident X rays. It increases with the atomic number, because heavy atoms contain more electrons than light ones. This secondary electron emission, correlated with fluorescence, is the emission of which I have tried to use the photographic action. Note that it is emission from the surface only, for matter absorbs electrons very much more strongly than it absorbs X rays.

To succeed in this aim, it is necessary that the sensitive emulsion (which is strongly pressed against the surface under experimentation, and is traversed first of all by the X rays) should not be affected by either the incident or the scattered X rays. It must, therefore, not absorb X rays more than to a weak degree, and must be sensitive to photoelectrons. To achieve the first requirement, it is necessary and sufficient that the emulsion be very thin and consist of fine grains which are not very sensitive, and that the X rays be "penetrating," *i.e.*, emitted from a high-voltage tube. Our emulsions are special films of the Lippmann type used in microradiography, the sensitive layer being only $\frac{1}{300}$ mm thick. I have had excellent results with Lippmann films manufactured by Kodak. One may also use high-contrast emulsions, and even silver-chloride photographic papers (Velox), very insensitive to X rays, but very sensitive to electrons (many of my experiments on electron-diffraction confirm this). As for the voltage, it should be between 100 and 200 kv, and preferably steady.

It is obvious that this method is entirely different from microradiography and from ordinary radiography by transmission, as is shown by FIGURE 1.

(1) Paradoxical as it may be, the photographic film is traversed by X rays first.

(2) The X rays do not act on the film but the electrons do.

(3) Penetrating X rays are used, as against the only slightly penetrating rays customary in ordinary microradiography.

(4) Only the surface-layer is studied, instead of the entire volume of the sample.

* This paper, illustrated with lantern slides, was the first first of two presented at the meeting of the Section on November 8, 1950.

† Director of the X-ray Laboratory of the National Center of Scientific Research, Bellevue, France; Professor at the Sorbonne.

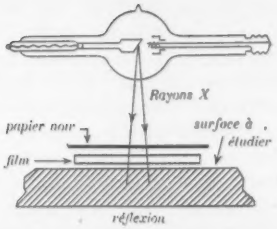

FIGURE 1. Radiography or microradiography by reflection. The sensitive side of the film is on the bottom, and placed against the surface of the sample under investigation (*papier noir*—black paper; *rayons X*—X-rays; *surface à étudier*—surface to be studied).

rays. Contiguity of film and surface is essential because of the strong scattering which photoelectrons suffer in air.

Results. I shall outline a few of the main applications, illustrating them with photographs.

(a) Study of surfaces of metals and alloys (FIGURE 2).

(b) Study of the topography of metal surfaces.

(c) Study of minerals. (FIGURE 3).

(d) Study of graphite in cast iron (FIGURE 4).

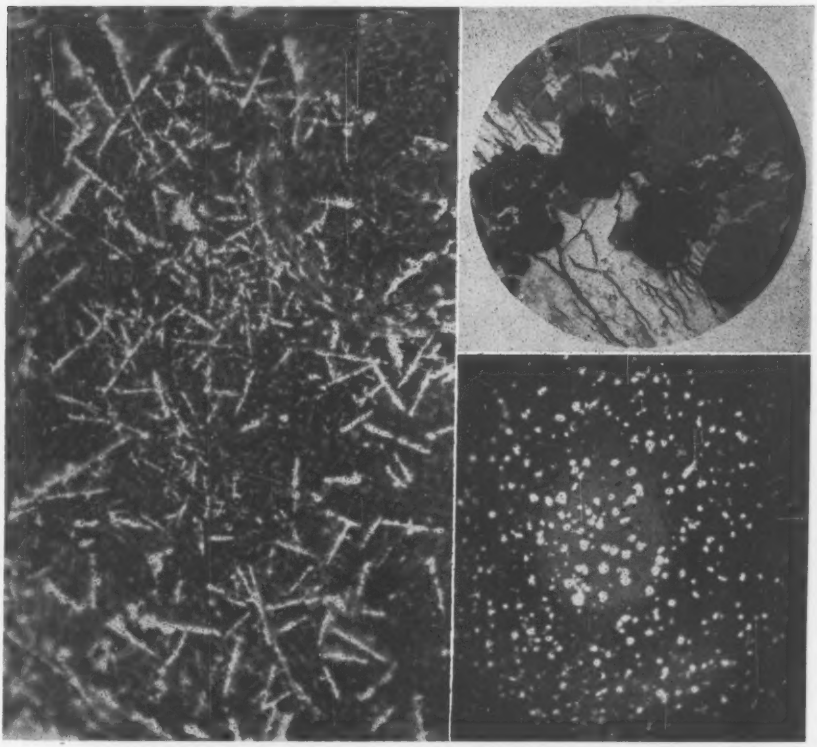

FIGURE 2 (*Left*). Electronic microradiography, by reflection, of an Al-Sb-Sn-Pb alloy.
FIGURE 3 (*Top right*). Electronic microradiography of pitchblende: black—uranium; gray—lead; white—SiO_2 (enlargement 6×).
FIGURE 4 (*Bottom right*). Electronic microradiography, by reflection, of cast iron (gray) containing graphite (white).

The experimental technique is very simple. The film is tightly squeezed against the surface under experimentation, and the whole thing is exposed to the X

Of course, many other developments are possible.

(B) *Electronic Radiography by Transmission.* We have observed that a heavy

metallic screen, one of lead for instance, when irradiated by hard X rays, emits photoelectrons of calculable energy which are able to traverse some thousandths or even hundredths of a millimeter of a thin object placed against the screen. This is the principle of electronic radiography by transmission, the object being traversed by the electrons, as in the electron microscope.

Many applications are feasible; *e.g.*, the study of papers, thin films, biological objects, *etc.* FIGURES 5 AND 6 give a sufficiently clear illustration.

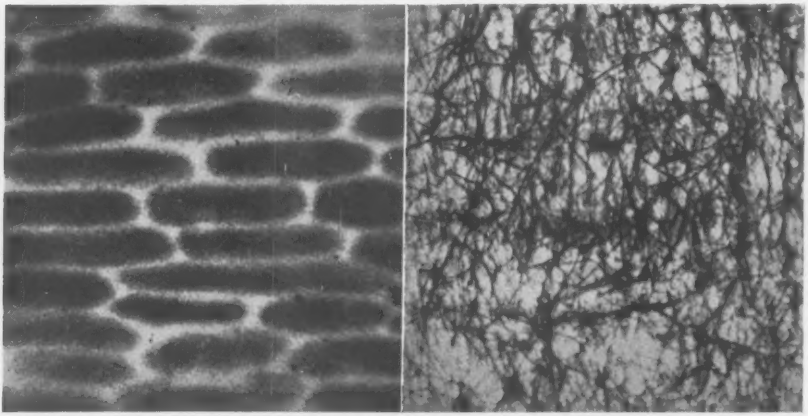

FIGURE 5 (*Left*). Electronic radiography, by transmission, of an onion peeling.
FIGURE 6 (*Right*). Electronic radiography, by transmission, of a sheet of cigarette paper (enlargement 8×).

In metallurgy, there can scarcely be any application of this principle, since the object has to be of a uniform and very small thickness. One might, however, attack the structure of detached films of oxides or sulphides.

We have, however, developed this method in a somewhat different direction, constituting, so to speak, a combination of electronic radiography by reflection and by transmission (FIGURE 7 and 8).

II. *Methods Based on the Diffraction of X Rays: Study of Surfaces*

It is known that the structure of surface films has an important connection with such phenomena as adsorption, corrosion, catalysis, lubrication, *etc.* Electron diffraction makes possible the study of surface layers some tens of millimicrons thick; X ray diffraction, by the classic method of "back-reflection," gives information about layers no thinner than 10 microns. There is no way of investigating layers of thickness intermediate between 0.1 micron and one or several microns. For these, X rays are too penetrating and there are not enough electrons. But this is just the range of thicknesses of peculiar interest for the phenomena of adsorption, oxidation, corrosion, lubrication, *etc.*

In my laboratory, M. Legrand contrived an ingenious way of solving this problem, the principle of which is as follows:

Consider a thin film laid down on the surface of a metallic support of microcrystalline structure, illuminated by a beam of X rays of wave-length λ. Choose the angle α between beam and surface so that one of the diffracted beams (*e.g.*, the 200 ray) shall emerge almost or quite parallel with the surface. Then the next beam, the 220 ray, for example, will make a somewhat greater angle with the surface. We are able to deduce this angle in advance from the crystal structure of the metal. The 200 ray will travel for a greater

distance in the thin film, and therefore will be more completely absorbed than will be the case for the 220 ray. By measuring the ratio of the intensities of these two beams; *e.g.*, by letting them fall on a photographic plate and measuring with a microphotometer, one can calculate the thickness of the film (its absorption-coefficient supposedly known). We were able to show that the method is valid by using it on films of cellophane laid in increasing number on the surface of a metal.

This procedure is feasible also with surface-films, be they amorphous or crystalline. We can measure the thicknesses of films of paint, varnish, or oxide layers (Al_2O_3, CuO, Fe_3O_4, *etc.*). Other methods will destroy the films.

If the surface-film is crystalline, it will give its own diffraction-pattern by which it can be identified. The smallness of the angle α makes it possible to observe the lines of the beginning of the spectrum (which are always the strongest), and the quantity of matter traversed by the X rays is greater than in the back-reflection method, wherefore more intense diffraction-patterns can be formed. Thus, one can show the structure of natural or artificial surface-films such as Al_2O_3 or its hydrates on aluminum surfaces; CuO or Cu_2O on copper surfaces, *etc.* This is possible even if the thickness of the oxide layer is very slight and the layer itself undetectable by other methods.

In summary, the method of M. Legrand makes it possible to prove the presence of a surface-film even thinner (in the best cases) than 0.1 micron, and to determine its thickness and its amorphous or crystalline structure (FIGURE 9). This shows how important it may be for various scientific and industrial problems.

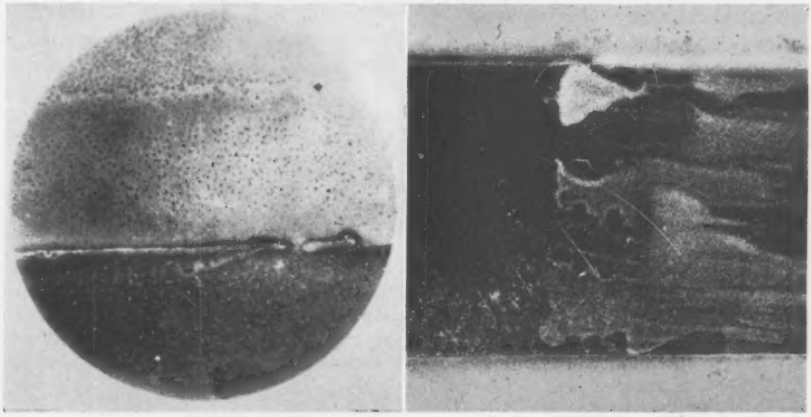

FIGURE 7 (*Left*). Bubbles of air in a film of paint of different thickness. The bubbles appear as black dots.
FIGURE 8 (*Right*). Layer of Aquadag on steel.

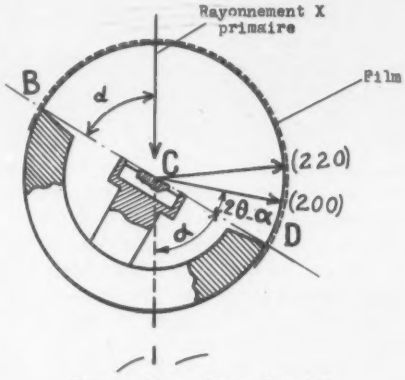

FIGURE 9. Schema of Legrand's method.

Naturally, M. Legrand worked out the complete theory, and showed that good diffraction-patterns could be obtained from thin films laid down on amorphous supports, such as glass. Examples of these are metallic films produced by sputtering or volatilization. It was possible to study the structure of films of gold and palladium of thicknesses of the order of 0.1 micron (700 Ångstroms for gold, for example).

In our laboratory, we made a camera particularly suitable for this purpose (FIGURE 10). By this camera, it is possible to study not only the surface-films deposited on metals, but the metal surfaces themselves. The angle of incidence has an important effect on the effective thickness of the layer in so far as this produces diffraction, and it is possible to show that, by varying this angle α, one obtains patterns corresponding to thicknesses which can be determined in advance. This permits, for example, the study of the phenomena of surface orientation, or of cold-working, and of size of grains as function of thickness. Of course, the classical "back-reflection" method does not permit such studies, or at least not with comparable precision. Our apparatus gives us beams so narrow that they show the doubling due to the fine structure of the K-alpha line, even for small angles of diffraction. This is shown by microphotometric traces of the films.

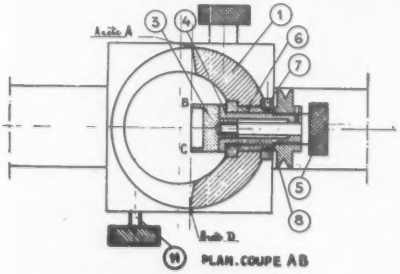

FIGURE 10. Simplified diagram of the camera.

Examples of applications of this method are as follows, and it is certain that many other problems could be treated with success:

(a) *Crystalline Layers on Amorphous Supports.* The method allows us to obtain the patterns of exceedingly thin films, down to a few hundred Ångstroms. Such is the case of metals deposited on glass.

(b) *Crystalline Layers on Crystalline Supports.* In the study of the oxidation of aluminum, it is important to know both how thick is the layer of alumina and whether it is amorphous or crystalline. Our method makes this possible. FIGURE 11 shows the pattern of the surface. One sees the lines of the metal and also those of a hydrate called boehmite. The thickness of the film was measured, and it amounted, in this case, to about 20 microns. FIGURE 12 shows a case in which the layer of alumina was amorphous. It is clear that the ratio of intensities of the 200 and 111 beams of the metal is altered by the presence of this very thin film of amorphous alumina, the 111 beam being much more completely absorbed than the 200.

(c) *Study of Oxidation Phenomena.* It is possible to follow with precision the phenomena of oxidation. We studied the formation of Cu_2O and CuO on copper simply heated to 200°, the oxide layers being only 2 or 3 microns thick.

(d) *Study of Alloys.* If one presses a copper foil strongly against an aluminum foil, while rolling these two foils simultaneously, an Al-Cu alloy forms on the aluminum and a different alloy (Cu-Al) forms on the copper. The thickness of the intermediate layer of alloy is very small, and yet the beams appear clearly and permit it to be identified (FIGURE 13). The narrowness of the beams permits the exact study of massive samples of alloys, of known or unknown constitution.

(e) *Surface Alterations of Metals by Cold Work.* Polishing of a metal is always attended with alterations in the surface layer. This question has long been studied, and no electron diffraction seems the best way of attacking it. Yet Legrand's method can yield interesting information about

surface-layers of thicknesses of the order of a few microns. One can thus follow the progress of the cold-working and the alteration of the grain as function of depth.

(f) *Quantitative Study of Mixtures.* Finally, the method can be used in the quantitative study of mixtures, because of the narrowness of the beams. We were able to determine, with exactness, the quantity of anatase (TiO_2) contained in samples of rutile (TiO_2). This is important for the paint industry, for rutile gives good paint and anatase does not.

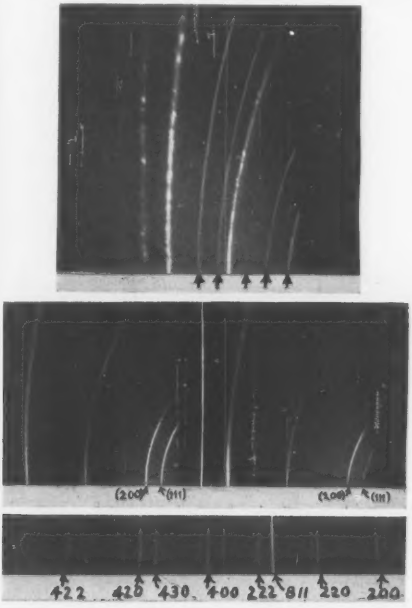

FIGURE 11 (*Top*). Aluminum and boehmite; lines of the boehmite are marked.

FIGURE 12 (*Middle*). Aluminum (left); aluminum with a superimposed layer of amorphous alumina (right). Note the ratios of the intensities of beams 200 and 111.

FIGURE 13 (*Bottom.*) Rolling of Al foil against Cu foil. Copper K-alpha lines. Angle of grazing incidence 42°. The unmarked lines are due to the alloy $CuAl_2$.

III. *Methods Based on Diffraction of Electrons: Study of Fresh Metal Surfaces and Their Properties.*

We wished to study the physico-chemical properties of "fresh" surfaces, prepared with avoidance of all contamination soiling or oxidation by the atmosphere. M. Courtel, in my laboratory, set up an apparatus permitting us to machine a sample of metal altogether inside of an electron-diffraction chamber previously evacuated. Immediately after the surface is prepared *in vacuo*, the sample is examined by diffraction of electrons at grazing incidence, and we get information about the clean surface. The various steps necessary to these processes are made by remote control. In our experiments, the machining of the surfaces was a grinding-process done by a grinder of diamond or glass. The fact that the motor which drives the tool is turning at speeds up to 45,000 r.p.m. brings about special problems of lubrication (FIGURE 14). The grinding may be done not *in vacuo*, but at pressures (of any gas) up to one atmosphere. In such cases, the surfaces are examined as soon as the chamber has been evacuated. There is no particular difficulty in subjecting the samples to various manipulations (heating, exposure

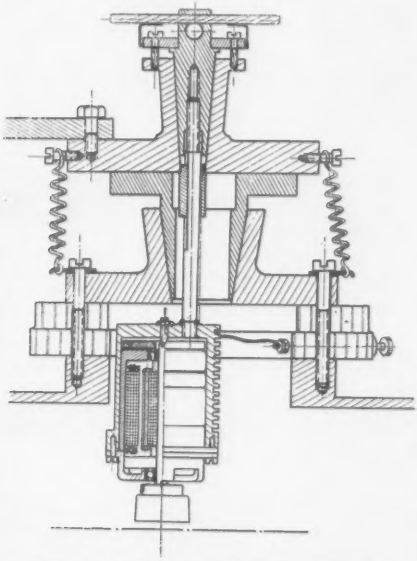

FIGURE 14. Cross-section of the grinding apparatus.

to other gases, *etc.*) after which they are again examined.

Our earliest observations pertained to the effect of gases on the clean surfaces, and even of small quantities of gases, organic vapors (which exhibit beautiful examples of oriented adsorption on surfaces of iron prepared *in vacuo*), oxygen of the air (which produces an immediate effect on cold iron), *etc.* Generally speaking, adsorption of gases reveals itself through a slight but perceptible alteration of the observed diffraction-pattern.

The new technique makes it possible to observe the preferred orientations produced on the surfaces of the samples by the grinding, which under ordinary conditions of experimentation are concealed by the effects of oxidation. There are three principal classes, one for body-centered cubic lattices, one for face-centered cubic and one for close-packed hexagonal lattices (the three most common types of lattice among metals). We observed these with various metals and alloys.

One of the important factors in grinding is the temperature to which the surface rises during the process. In the particular case of cobalt, we were able to infer this from the changes in structure observed by diffraction in the surface-layer (this observation cannot be made when the grinding is done in air, because of oxidation). We found that this surface-temperature is 400°C. for a peripheral speed of 15 meters/sec. of the mill; it increases with the speed (FIGURE 15). To explain certain phenomena observed during this investigation (persistence at low temperatures of crystal phases stable at high temperatures, spontaneous magnetization, *etc.*), we postulated a specially rapid quenching of the layers which had been exposed to the grinding.

Thus, the phenomena of oxidation of a fresh metal surface can be investigated with precision. It is sufficient to prepare a surface *in vacuo*, introduce oxygen at the desired pressure (which may be very low), and then examine the diffraction-pattern after restoring the vacuum. Of course, the sample can be simultaneously heated to a chosen temperature. We can observe with much exactness the successive appearance of various oxides. The same technique is applicable to other surface oxides. The same technique is applicable to other surface reactions, such as sulphuration.

As an example of a contribution of the new technique to chemistry, I take the case of cerium. By grinding in a very rarefied oxidizing atmosphere, we could produce and detect the sesquioxide Ce_2O_3, of cubic form, at the very surface of the ground metal. Hitherto, the existence of this crystalline form of the oxide had not been proved. Other features of the oxidation of this metal could be observed when the grinding was performed in atmospheres of pure oxygen or pure nitrogen.

In summary, the technique of grinding in rarefied and controlled atmospheres, combined with the study of the resulting surfaces by electron-diffraction, is both a new method of research in the physics of metal surfaces, and a special type of surface-chemistry process. Thus, it is possible to distinguish the physico-chemical factors (adsorption, chemical affinity, *etc.*) from the structural factors. This helps

FIGURE 15. Dependence of the surface-structure on the speed of grinding. Single crystal of hexagonal cobalt. Left: hexagonal lattice, speed 10m/sec. Middle: hexagonal and cubic lattices speed 20m/sec. Right: cubic lattice, speed 30m/sec.

greatly in clearing up their respective roles in producing the final state of the surface.

The increase of chemical or of physico-chemical activity of fresh surfaces of solids, the results of machining metals, the measurement of surface-temperatures, the phenomena of oxidation, *etc.*, can be investigated by this method, which seems to us to be important both in science and in technology.

RECENT PROGRESS IN NEUTRON DIFFRACTION*

By C. G. SHULL†

During the past several years, neutron diffraction techniques have advanced to the stage where they now can be applied to many of the problems which are ordinarily studied with X rays and electrons. This has been possible, of course, because much more intense beams of slow neutrons have become available from nuclear reactors or piles. Most research has been done to date with the Debye-Scherrer powder diffraction technique in which a polycrystalline scattering sample is illuminated with a collimated beam of monochromatic neutrons (wavelength about one angstrom). The resulting diffraction pattern can be suitably detected with a neutron counter and can be interpreted in terms of the crystalline structure of the scattering sample. Unfortunately, there is no known source of monochromatic neutrons of this desired wavelength, but it is possible to obtain such radiation by crystal monochromatization of the neutron radiation which is available from the pile. Large single crystals are used for this purpose, usually one of the metallic elements, such as copper or lead.

The general fields of application of the neutron diffraction technique can be grouped into three classifications: (a) nuclear physics studies, (b) crystallographic studies, where the neutron scattering amplitudes may be more suitable than the equivalent X ray ones, and (c) magnetic structure studies. The present report will summarize in a very abbreviated fashion some of the research which has been performed during the past three or four years in this field and which illustrates or exemplifies each of these classifications.

Nuclear Physics Studies. Since neutron scattering by an atom is usually a completely nuclear scattering process, with the electrons in the outer part of the atom offering little or no contribution, one obtains information about the neutron-nucleus interaction. This interaction arises because of the very strong, yet short-range, nuclear force field, about which very little is known. Up to the present time, the scattering properties of about 60 elements or specific nuclides have been studied and it is to be hoped that these will be of assistance in the eventual formulation of a nuclear theory. Such measurements have shown, for example, that spin dependent scattering is more the rule than the exception. In other words, the nuclear force field which the neutron experiences in approaching a nucleus is strongly dependent upon the relative orientations of the neutron and nuclear spins. The scattering data have also been of interest in frequently showing the effects of resonance scattering by the nucleus. Both the magnitude of the scattering cross section and the algebraic sign of the scattering amplitude are of interest in connection with nuclear resonance scattering, and the data show that effects of this type are frequently being encountered in the neutron energy region under consideration. Taken all together, the neutron scattering cross sections show very little regularity from nucleus to nucleus, and this is in pronounced contrast to the relative values of X ray scattering cross sections, where great regularity is to be found as one progresses from light atoms to heavy atoms. Nuclear size or mass appears to have very little correlation with the efficiency of neutron scattering.

Crystallographic Studies. One of the

* This paper, illustrated with lantern slides, was the second of two presented at the meeting of the Section on November 8, 1930.

† Oak Ridge National Laboratory, Oak Ridge, Tennessee.

most interesting applications of the neutron diffraction technique in crystallographic studies has been that of hydrogen atom location in crystal lattices. The solution of such problems by conventional X ray diffraction methods has been well-nigh impossible because of the very weak scattering of X rays by a hydrogen atom with its single electron. The other atoms in a hydrogen-containing material scatter X rays so much more strongly that it becomes very difficult to interpret the diffraction pattern in terms of the hydrogen atom location in the lattice. This is not the case with neutrons, where the hydrogen nucleus scatters comparably with other nuclei. A number of structural problems of this type have been studied, including those of the simple alkali hydrides, ice, KHF_2, and several heavy metal hydrides. In NaH, for example, it has been found that the structure is isomorphous with that of NaCl. The hydrogen atom merely replaces a chlorine atom. A more complicated structure is indicated in the case of the structure of ice, where the hydrogen atom apparently can reside at either of two positions along the O-O bonds making up the lattice. This disordered ice structure is in accordance with conclusions drawn by Pauling from residual entropy data obtained at low temperatures. In contrast to the ice data, the hydrogen atoms in KHF_2 reside at the centers of F-F linkages, according to recent studies by Levy and Peterson in this laboratory. Results such as these are of considerable interest to structural chemists, who are anxious to explain the hydrogen-bond peculiarities which frequently are found.

Problems similar to those of hydrogen atom location are frequently encountered in structural studies of heavy metal oxides and carbides. Again, the neutron scattering amplitudes may be more favorable for a complete structural analysis, such as in cases of various oxides and carbides of uranium and thorium. A number of such studies have been performed. An interesting study of the opposite type is encountered in order-disorder investigations of Fe-Co alloys. The neutron scattering amplitudes for Fe and Co differ sufficiently so that the ordered superstructure which is sometimes obtained with this alloy can be easily detected. This is very hard to do with X radiation, since these neighboring atoms in the periodic system scatter so similarly.

Magnetic Structure Studies. It has been mentioned in an earlier section that, ordinarily, the scattering of a neutron by an atom is accomplished primarily by the nuclear force field. This is not true, however, for the scattering by atoms which possess a magnetic moment, since there will be an additional interaction between the neutron magnetic moment and the atomic moment. If the scattering atom has a full complement of electrons in its quantum shells, it will possess no magnetic moment, but if some of its shells are incomplete, such as in the transition elements and in the rare-earth elements, strong magnetic moments can exist. In many cases, the magnetic scattering of a neutron can exceed the purely nuclear scattering. Substances possessing atoms with magnet moments are classified as paramagnetic, ferromagnetic, or antiferromagnetic materials, depending upon the type of interaction which exists between neighboring atomic moments in the lattice. In a paramagnetic substance, neighboring moments are uncorrelated with respect to each other, whereas in ferromagnetic and antiferromagnetic substances, neighboring moments are locked into parallel and antiparallel orientation, respectively, by the very powerful quantum exchange forces. The neutron diffraction patterns characteristic of these three different magnetic lattices will differ pronouncedly, so that it will be possible to identify details of the magnetic lattice from the features of the diffraction pattern.

An illustration of this is given in the patterns obtained for MnO at various temperatures. At temperatures of 100 or 200° C, one obtains only magnetic diffuse scattering, which is characteristic of a

paramagnetic lattice with uncorrelated moments. As the temperature is lowered to room temperature and below, the magnetic pattern takes on residual coherence, much like the X ray patterns obtained for liquids, and this signifies the introduction of short range magnetic order in the lattice. In the vicinity of 120° K, very strong magnetic diffraction lines appear, indicating that the moments have become locked into fixed orientation with respect to each other. From studies of the intensity and position of these new diffraction lines, a detailed picture of the magnetic lattice can be obtained, just as in usual crystallographic studies, where atomic positions are located in the lattice.

A number of substances which possess interesting magnetic structure have been investigated by these techniques. For example, ordinary hematite, α-Fe_2O_3, has been shown to have a rigid magnetic structure at room temperature. Undoubtedly, many materials possess subtle magnetic structures of various types and it can be expected that extensive neutron diffraction research will be directed along these lines in the future.

SECTION OF BIOLOGY

COMPARATIVE BIOLOGICAL EFFECTS OF CORTISONE ACETATE AND KENDALL'S COMPOUND F ACETATE: A PRELIMINARY REPORT*†

By DAVID A. KARNOFSKY‡

In the course of testing various chemicals for their effects on the development of the chick embryo, 11-dehydro-17-hydroxy-corticosterone acetate (cortisone acetate)§ was found to produce a remarkable and characteristic inhibition of growth.[1] Cortisone was injected into the yolk sac of the 4-day embryo and, when sacrificed at 18 days of age, the embryo was small, feather formation was almost absent, and the extra-embryonic membranes were markedly restricted. The characteristic growth-inhibiting effect of cortisone appeared to be initiated at the 8th day of incubation for, prior to this period, the embryo grew at an approximately normal rate in the presence of cortisone. The events, occurring during the 8th to 10th days of development, which make the embryo susceptible to the action of cortisone, and the morphological and metabolic changes resulting from the effects of cortisone, are problems of great interest. Cortisone was most active when injected onto the chorioallantoic membrane of the 8-day embryo, and the *minimum effective dose* of cortisone which produced an arbitrarily defined degree of growth-inhibition in the embryo sacrificed at 18 days was 0.45 mg. per egg.

The striking effect of cortisone on the chick embryo led to the assay of other adrenal cortical steroids for a similar type of activity. Kendall's Comp. F acetate (17-hydroxy-corticosterone acetate), applied to the chorioallantoic membrane of the 8-day embryo, produced the same type of effect as cortisone on embryonic development, but the minimum effective dose was 0.015 mg. per egg, Comp. F thus being about 30 times more active by weight than cortisone on the chick embryo.

It was, therefore, of interest to determine whether the high activity of Comp. F as compared to cortisone obtained in the

* On November 13, the Section of Biology held a symposium entitled "Recent Experimental Studies with ACTH, Cortisone, and Related Steroids." This paper, illustrated with lantern slides, was the second of five presented at the symposium. The abstracts of two of the other papers, "The Effect of Cortisone and ACTH on Experimental Scurvy in Guinea Pigs," by G. A. Hyman and "The Effect of Adrenal Cortical Steroids and Diet on Tumor Growth," by G. A. Emerson, have not been received.

† This study was aided by the American Cancer Society, the National Cancer Institute of the United States Public Health Service, the Damon Runyon Memorial Fund for Cancer Research, and the Babe Ruth Cancer Fund of the American Cancer Society.

‡ Division of Experimental Chemotherapy, Sloan-Kettering Institute, New York.

§ We are grateful to Merck and Company for providing cortisone acetate and to the Upjohn Company for providing Compound F acetate. Compound F differs from cortisone only in having a hydroxyl instead of an oxygen group in the 11-position on the steroid molecule.

newly-hatched chick. Groups of White Leghorn chicks, one-day old and weighing about 40 to 45 grams, were injected intramuscularly daily for 10 days at several dose levels with cortisone acetate and Comp. F acetate. At the highest dose, 10 mg. per day of cortisone, marked retardation of growth occurred, so that the chicks gained an average of 18 per cent over their original weight during the 10-day period of injection, as compared to 130 per cent weight gain for the controls. Comp. F acetate, at a dose of 0.1 mg. per day for 10 days, produced the same degree of growth inhibition as 10 mg. per day of cortisone acetate. The cortisone- and Comp. F-treated chicks and controls were sacrificed two days after the last injection, at 12 days of age, and the liver, spleen, adrenal, heart, and comb were weighed. These weights are given in TABLE 1. Microscopic sections of the adrenal glands of the cortisone- and Comp. F-treated chicks showed a marked decrease in adrenal cortical tissue and a relative increase in the chromaffin cells. These data indicate that the two hormones had similar effects in the young chick, but Comp. F is 50 to 100 times more active than cortisone.

TABLE 1

AVERAGE BODY AND ORGAN WEIGHTS OF CHICKS TREATED FOR 10 DAYS WITH CORTISONE ACETATE OR COMPOUND F ACETATE (12 DAYS OLD WHEN SACRIFICED)

	Control	*Cortisone Acetate*	*Comp. F Acetate*
No. chicks	14	13	13
Dose	—	5 mg./day × 10	0.1 mg./day × 10
Body wt. gms.	92.3	58.1	55.9
Liver, wt. gms.	4.48	3.48	3.70
Spleen, wt. mg.	113.3	54.6	40.2
Adrenals, wt. mg.	23.8	16.8	14.7
Heart, wt. mg.	751	584	538
Comb, wt. mg.	42.7	31.2	15.4

This observation raised the question as to whether the marked difference between cortisone and Comp. F in the chick also existed in a new-born mammal. New-born mice of the AKM strain were injected subcutaneously daily for 10 days with cortisone acetate. One-tenth of a mg./day was usually fatal within 10 days, and the LD_{50} in animals surviving until the 10th day was 0.05 mg./day. This dose produced characteristic changes in the mouse. There was a moderate decrease in growth, hair formation was strikingly diminished, and there was premature eruption of the teeth on the 7th day, whereas those of the controls erupted on the 10th day. The treated mice sacrificed on the 11th day had an atrophic thymus and adrenal cortex, and a small spleen. Sections of the skin showed a remarkable reduction in the number of hair follicles. Those mice permitted to survive beyond the 10-day injection period did not immediately recover from the effects of cortisone, and their subsequent development is now under study. Comp. F acetate also produced effects identical to cortisone in new-born mice and at approximately the same dose.

Discussion and Conclusions. Cortisone and Comp. F have remarkable and specific effects, similar and perhaps identical, on the growth and development of embryos and newly-born animals.

In new-born mice, cortisone and Comp. F have the same degree of activity by weight, an observation consistent with the approximately equal activity of these hormones when assayed by several different techniques in mammalian systems.[2] The effective dose of these hormones in new-born mice is 0.05 mg./day for 10 days [this is calculated to be about 25 mg./kg./day × 10 (250 mg./kg. for the total course)].

In the chick embryo, Comp. F is 30 times, and in the newly hatched chick, about 100 times more active than cortisone. The effective dose of Comp. F acetate is 0.10 mg./day × 10 days [this is calculated to be about 2 mg./kg./day × 10 (20 mg./kg. for the total course)] as compared to a dose of cortisone acetate with equivalent biological activity, 10 mg./day × 10 [calculated to be about 200 mg./kg./day × 102 (000 mg./kg. for the total course)]. On a mg./kg. basis, in the chick, Comp. F is about 10 times more active and cortisone about 1/10 as active, when compared with their effective dosage in mice.

The significance of the marked activity of Comp. F in the chick is enhanced by the evidence that Comp. F is believed to be the hormone normally secreted by the adrenal cortex.

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THE RELATIONSHIP OF VARIOUS STEROIDS TO THE THYROID, ADRENALS, AND LYMPHOID SYSTEMS OF THE NORMAL RAT*†

By WILLIAM L. MONEY‡

Many studies, both clinical and experimental, have recently shown that the administration of ACTH, or cortisone, to the normal animal produces dramatic changes in body function. We have been investigating for some time certain effects of various steroid treatment on the normal rat. The factors we have studied are as follows: (1) the influence of steroids to thyroid function, (2) the influence of steroid compounds on the adrenal glands, and (3) the effect of these substances on the lymphoid system.

We have treated normal adult male rats with various steroids for a period of 10 days. At the time of autopsy, the effect of the compounds on thyroid collection of radioactive iodine, adrenal size, and the weight of the thymus and lymph nodes have been determined.

It has been found that both ACTH and cortisone produce a marked depression in the amount of radioactive iodine collected by the thyroid gland. This is, however, not a characteristic limited to these compounds. Compound A, desoxycorticosterone, and some of the estrogens also prevent the thyroid from collecting normal concentrations of radioactivity. On the other hand, some steroids have been observed to increase the collection of I^{131} found in thyroid tissue. Testosterone, progesterone, dihydro E, and some other steroids have the capacity to increase the amount of radioactive iodine which the thyroid will concentrate.

It is well known that ACTH when administered to normal animals produces a great increase in the size of the adrenal glands. Although the response is not nearly so marked, testosterone, most of the estrogenic hormones, and Compound L also have been found to produce adrenal hypertrophy. Cortisone, Compound A, and desoxycorticosterone injections, however, all result in atrophy of the adrenal glands.

Nearly all of the steroids which we have studied have been found to produce depression of the thymus. Progesterone appears to be the only compound which increases significantly the size of this organ.

We have observed lymphoid destruction in animals treated with cortisone, Compound A, and ACTH. Some steroids have been shown to increase lymph tissue slightly. The most dramatic increase in lymph node size has been observed with Compound L.

These studies indicate that some steroids act essentially like ACTH and cortisone. Others have an entirely different effect or are ineffective in producing changes on the end organs we have been investigating. The mechanisms of action of some of these compounds are being further studied.

Acknowledgements

21-Acetoxy-pregnenolone, progesterone, and α-estradiol benzoate were furnished by Dr. Edward Henderson, Schering Corporation, and ACTH by Dr. John Mote, Armour Laboratories. Cortisone and dihydro E were furnished by Dr. Randolph Major of Merck and Co., and Dr. Irving Sollins of Syntex Company furnished Compound L.

* On November 13, the Section of Biology held a symposium entitled "Recent Experimental Studies with ACTH, Cortisone, and Related Steroids." This paper, illustrated with lantern slides, was the third of five presented at the symposium.

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SOME EFFECTS OF CORTISONE AND RELATED STEROIDS ON TRANSPLANTED TUMORS OF THE MOUSE*

By GEORGE W. WOOLLEY† ‡

In the present report, the technique of adrenal cortical hormone administration on mouse lymphoid tumor growth is emphasized. It is shown that oral administration is not as efficient as subcutaneous injection of a saline suspension of cortisone when a dose of 1 mg./day is used in two divided doses, but that the efficiency of oral administration may be increased by multiple feeding.

It may be that one would never want to give cortisone orally in animals or in man, but we do want to find out how efficient it is by this method. It is well known that steroid substances differ in relation to method of administration. Estriol is a less effective estrogen than estrone if subcutaneous injection is used, but more effective when oral administration is employed. Methyltestosterone has been a very helpful oral androgenic hormone. What about the adrenal cortical steroids? A whole field of investigation is possible.

It has been shown by Heilman and Kendall[1] and subsequently by other workers that 11-dehydro-17-hydroxy-corticosterone (Compound E, cortisone) has a restraining effect on lymphoid tumors. Such tumors even regress markedly in their first encounters with this substance. Other early experiments by Murphy and Sturm,[2] Law and Speirs,[3] and by Diller, Beck, and Blauch,[4] showed that tumor growth was either retarded or partially regressed by adrenal cortical extracts. It is also known that growth of such diverse tumor types as rhabdomyosarcoma, neuroblastoma, and mammary gland carcinoma of experimental animals may be reduced in growth rate by the use of cortisone. There is also evidence in experimental animal studies that several cortical steroids are nearly as efficient as cortisone in modifying tumor growth, if used in suitable amounts. For example, one might cite Compound F, Compound A, or corticosterone (Kendall's Compound B). In our experience, compound E and compound F require almost equal amounts, and compound A and corticosterone two to three times as much as for compounds E and F, if given in single, daily, subcutaneous injections.[5]

C3H mice bearing lymphosarcoma 6C3-HED were employed. The transplantable lymphosarcoma was transferred by injection of the local tumor mass with a 12-gauge trochar. Investigations were over 10-day periods. The following criteria were employed in determining the response of the mice to the treatment: (1) tumor weight, (2) body and organ weights, (3) hematologic studies, and (4) histologic studies.

A summary of the first oral* and subcutaneous injection experiment is as follows:

	Subcutaneous	*Oral*
Number of mice	6	7
Mean body wt. (change in gms.)	−0.7	+0.1
Mean wt. tumor mass in gms.	0.4	1.4

Other details are shown in TABLE 1. It can be seen that subcutaneous injection was more effective in holding down tumor size, body weight, weight of lymphoid organs, adrenal, RBC, WBC, and the per cent of lymphocytes, monocytes and eosinophils in the WBC. A simple fat stain showed more fat in the livers of the subcutaneously- than in the orally-treated mice. In TABLE 1, it may also be noted that each treatment tended to be more effective in females than in males. This is in line with evidence we have secured which indicates that tumor 6C3HED grows to a larger size in a 10-day

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* Placed at the base of the tongue by means of a syringe and an 18-gauge bent needle with a ball point.

period in male than in female mice. Recent data is as follows:

	Males	*Females*
Number of mice	8	6
Mean body wt. (change in gms.)	+5.6	+3.6
Mean wt. tumor mass in gms.	2.4	2.0

TABLE 1

COMPARATIVE ORGAN WEIGHTS AND HEMATOLOGIC FINDINGS FOR MALE AND FEMALE MICE TREATED WITH SUBCUTANEOUS AND ORAL CORTISONE INJECTIONS. ONE MG./DAY IN TWO DIVIDED DOSES WITH AUTOPSY ON THE 10TH DAY

	Subcutaneous		*Oral*	
No. of mice	3♀♀	3♂♂	4♀♀	3♂♂
Mean body wt., start	21.8	19.2	19.4	24.1
Mean body wt., autopsy	21.0	18.5	18.4	25.7
Mean wt. tumor mass	0.2	0.6	1.1	1.9
Mean organ wt. (% body wt. less tumor)				
Adrenal	0.013	0.010	0.019	0.008
Thymus	0.008	0.011	0.013	0.016
Spleen	0.30	0.24	0.29	0.35
Mesenteric nodes	0.063	0.056	0.088	0.149
Liver	6.6	6.2	5.4	6.6

Hematologic findings (2♀♀ in each class)

	Subcutaneous		*Oral*	
	24 hr.	*240 hr.*	*24 hr.*	*240 hr.*
Mean total RBC × 10^6	9.6	4.6	10.0	7.2
Mean total WBC × 10^3	3.1	3.7	3.5	5.4
Mean % lymphocytes	25	4	40	17
Mean % monocytes	6	2	5	6
" " neutrophils	68	94	53	77
" " eosinophils	1	0	2	0

Hematologic findings (3♂♂ in each class)

	Subcutaneous		*Oral*	
	48 hr.	*240 hr.*	*24 hr.*	*240 hr.*
Mean total RBC × 10^6	8.7	7.2	10.2	8.5
Mean total WBC × 10^3	1.7	1.6	1.02	5.5
Mean % lymphocytes	14	8	34	25
Mean % monocytes	0	0	5	1
" " neutrophils	86.0	92.0	59	74
" " eosinophils	0.0	0.0	2	0.0

The above findings may be related to the anabolic action of the male hormone. Heilman and Kendall with a lymphosarcoma, and Diller *et al.* with sarcoma 37, noted the regressing action of steroids was much more pronounced in female than in male mice.

Other methods of treating animals or individuals with steroid preparations have been or might be tried, *i.e.*, intramuscular, intraperitoneal, or local application; use of steroids of various solubilities; or the use of pellet forms. For oral use, the dose might be increased. We did try more frequent oral administration, that is, once every three hours throughout the 24 hours,

TABLE 2

COMPARATIVE ORGAN WEIGHTS AND HEMATOLOGIC FINDINGS FOR MALE AND FEMALE MICE TREATED WITH SUBCUTANEOUS AND ORAL INJECTIONS OF CORTISONE. ONE MG./DAY IN 8 DIVIDED DOSES WITH AUTOPSY ON THE 10TH DAY AFTER THE 1ST INJECTION

	Subcutaneous		*Oral*	
No. of mice	1♀	1♂	2♀♀	1♂
Mean body wt., start	16.0	18.5	19.9	18.0
Mean body wt., autopsy	16.0	16.1	17.8	17.0
Mean wt. tumor mass	0.03	0.0	0.03	0.10
Mean organ weight (% body wt. less tumor)				
Adrenal	0.015	0.014	0.017	0.010
Thymus	0.010	0.012	0.014	0.018
Spleen	0.26	0.20	0.20	0.20
Mesenteric nodes	0.064	0.236	0.089	0.058
Liver	7.2	7.0	6.3	6.8
Hematologic findings (24 hr.)				
Mean Total RBC × 10^6	7.9	8.1	8.2	9.2
Mean total WBC × 10^3	0.6	1.2	2.4	1.5
Mean % lymphocytes	26	8	27	44
Mean % monocytes	2	5	4	4
Mean % neutrophils	72	87	69	52
Mean % eosinophils	0	0	0	0
Hematologic findings (240 hr.)				
Mean total RBC × 10^6	6.4	6.0	8.2	11.1
Mean total WBC × 10^3	1.9	3.3	1.5	1.0
Mean % lymphocytes	9	3	1	2
Mean % monocytes	0	1	0	0
Mean % neutrophils	91	96	99	98
Mean % eosinophils	0	0	0	0

rather than twice daily. The results of a preliminary experiment are shown in TABLE 2. The summary is as follows:

	Subcutaneous	*Oral*
Number of mice	2	3
Mean body wt. (change in gms.)	−1.2	−1.8
Mean wt. tumor mass in gms.	0.01	0.05

It is evident that tumor growth has been restricted more than with twice daily injections. Also, there is evidence that the WBC count, and the per cent of lymphocytes, monocytes, and eosinophils are reduced to a greater extent. One possible helpful point was that the RBC count was not as greatly reduced in the oral as with the subcutaneous treatment in either experiment 1 or 2.

Additional studies are being made with 17-alpha,hydroxy-21-acetoxy pregnane-3, 11,20-trione, a substance similar to cortisone but reduced in the A ring. Ten mg./day were used in two divided doses. The results were as follows:

	Subcutaneous	*Oral*
Number of mice	5	6
Mean body wt. (change in gms.)	+0.1	0.0
Mean wt. tumor mass in gms.	1.2	1.6

The above substance, at the dose level given, had less tumor growth inhibiting action than cortisone acetate when the latter was given 1 mg./day in two divided doses. When compared to the control animals discussed above, it is evident that this steroid substance modifies tumor growth to some extent. As with cortisone acetate, the effect is also reflected in the lymphoid tissue of the animal as determined by organ weight, and in the blood picture. An interesting point, if confirmed by further experimentation, is that there is less loss of activity with oral as compared to subcutaneous use than with cortisone acetate. This perhaps indicates one important region of activity loss with oral administration of cortisone acetate.

Summary. Evidence has been presented that cortisone acetate has activity in reducing the rate of lymphoid tumor growth when given orally, but that it is not so effective as when applied by subcutaneous injection in equal amounts. Preliminary evidence is presented indicating that the tumor growth restricting activity may be increased by feeding at 3-hour intervals, over twice daily feeding. Also, that a form of cortisone reduced in the A ring has less loss of activity upon oral use than the more effective cortisone acetate.

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SECTION OF MATHEMATICS AND ENGINEERING

TECHNOLOGICAL STABILITY IN INDUSTRIAL OPERATIONS*

By SEBASTIAN B. LITTAUER†

The concept of technological stability entails an extension of the concept of statistical stability to the whole manufacturing procedure. Industrial operations, the operations by which our present industrial society provides for its material need, are, for the most part, operations of mass manufacture. An operation is comprised of many aspects, among which, three, production method, production rate, and product

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quality, rather completely characterize the operation. Since the earliest days of man, artisans, engineers, and industrial administrators have undertaken the development of these aspects of manufacture with the general aim of getting more output, in some sense, for a given input. The most highly publicized and most widely practiced of these techniques have been ascribed to Frederick W. Taylor, who began his work more than a half century ago. In 1909, he presented a system of ideas in a paper entitled "Principles of Scientific Management" to the American Society of Mechanical Engineers.[1] Taylor's views, which he referred to as "Scientific Management," emphasized the planning of productive effort in such a manner that the outcome, output, of this effort was predictable in terms of quantity. Although Taylor also had in mind the quality of product, in perhaps some vague sense, he was primarily concerned with predictions of output. He stressed the elementizing of operations and modifying methods so as to insure the most highly productive operation. He further stressed the elimination of worker initiative and he proposed manufacturing procedures, the better to guarantee high output of mass production operations. While Taylor did stress wage payments in relation to rates of output, he seemed primarily concerned with establishing standardized rates of productivity. And, in spite of his stress on standard production methods and his monumental technical job in "The Art of Cutting Metals,"[2] the heritage of his influence is largely to be found in the plethora of persons in industry engaged in setting up rates of production based in part on time measurements, in part on individual judgment, and, in part, on collective bargaining.

A contemporary of Taylor was Frank N. Gilbreth, who, together with his wife, Dr. Lillian M. Gilbreth, developed facility in modifying methods of production to a high degree by examining closely the motions performed by a worker in carrying out a productive operation. The Gilbreths are known primarily for "motion study,"[3] which was calculated to produce the highest output within a given time. It may be well, however, to say that, for a half century or more, efforts have been directed by disciples of Taylor and the Gilbreths to the standardization of production. Whatever ultimate aims these propounders of techniques for "efficient" manufacturing procedures had in mind, they were concerned with devising "methods" by which one could predict maximum output for given input. For the purposes of this discussion, I must say that they have been concerned with the aspects of manufacture affecting production methods and production rates, with some vague notion of the "one best method" and of a so-called "fair day's work."

A little more than a quarter of a century ago, Walter A. Shewhart of the Bell Telephone Laboratories concerned himself with the problem of product quality in mass manufacture. Product quality has definite bearing on output in that a product which does not conform with design specifications can not be counted in the output. At any rate, product that is scrapped or requires rework reduces the overall production rate. Also, if considerable examination of product is necessary, the overall man-hour input for accepted product output is thereby increased. Shewhart[4, 5, 6] recognized the fact that attainment of specified product quality is a fundamental problem of scientific method, a problem of prediction. Accepting the principle of causality and recognizing the inherent variability in repetitive procedures, he formulated a system of ideas which yielded operationally verifiable criteria for the attainment of specified product quality. Furthermore, he recognized that such criteria can be established only within the framework of an accepted goal or set of constraints. This goal is essentially economic in nature, in that the input in terms of cost shall not be greater than the value of the output in equivalent measures of value of the gain in quality.

Shewhart formulated an operational definition of quality so that quality can be measured in verifiable terms. He asserted

that prediction within limits is possible when a "constant system of chance causes" exists. Chance causes of variation are defined as unknown causes or causes which it is not economic to discover. He termed as "assignable causes" of variation those causes which can be economically identified and eliminated. In order to be able to establish criteria for the discrimination between chance and assignable causes, it is necessary to implement principles of statistical inference and the associated precepts of probability.

It is not possible, within the space of this article, to give an extended discussion of the science of statistical quality control which Shewhart propounded. Fundamental to the attainment of quality, to the attainment of a state of the production process wherein it is possible to predict within specified limits the quality that will be realized, is the following three-step continuing sequence: quality specification, quality production method, and quality evaluation. One may recognize here the parallel with the usually accepted three steps in experimental inference; hypothesis, experiment, and test of hypothesis. The quality specification is the hypothesis of the quality to be obtained. The production method, or the process, is equivalent to the experiment whose results are to be examined in order to determine whether the hypothesis is verified in fact. The results of the production process are inspected or tested and these inspection or test results evaluated to determine whether the specified quality has been attained. Until quite recently, statistical inference has only rarely been employed in testing hypotheses in the engineering sciences. Criteria of acceptance of physical hypotheses have usually been at the disposal of the judgment of the individual engineer or scientist. While manufacture and scientific inquiry are quite parallel in respect to experimental inference, the requirements of attaining quality in mass production differ, in that failure may destroy the manufacturing activity. The failure to attain predicted quality may mean such loss as to prevent further manufacture. The three-step sequence in attaining industrial quality, therefore, must be continuing and self-corrective and must lead to the realization of a constant chance cause system in the production process whereby the desired quality can be assured. One of the facts of nature eminently verified in industrial practice is that a system of causes operating in a production process is not constant at the outset, and usually contains one or more assignable causes of variation. It is necessary, therefore, to have available simple criteria for the detection of the presence of assignable causes of variation. Knowledge of the presence of assignable causes provides the signal for starting the hunt for such causes and their essential elimination. When the cause system is constant for a reasonable period of time during the operation of a production process, we may predict that the future results of the process will lie within specified limits.

For an example of this principle, consider the manufacture on an "automatic" of a cylindrical metal part. The causes of variation in the diameter of this part may grow in magnitude or they may come and go because of variation in raw material, sharpness of tools, or wear of machine components. In a short space of time, however, it is common experience that new assignable causes do not develop. If we then inspect a few pieces made in succession, such variation as occurs in the diameter of this part, for example, may be attributed to intrinsic causes, *i.e.*, to the existing chance cause system. If successive samples are taken at intervals sufficiently spaced in time, variation resulting from new causes can enter the cause system. If we examine 25 samples, each containing 5 elements, there is a within variation for each sample, and there may also be a variation between samples which differs in magnitude from the within variation. We can estimate the standard deviation which belongs to the constant system of chance causes from the samples themselves. If there are no assignable causes, then the arithmetic means

TABLE 1
RECORD SHEET FOR MEASUREMENTS

Material or product: bushing
Characteristic measured: length
Unit of Measurement: 1/1000 inch difference from 1"
Date recorded by:

Series No.	Date (and hour) produced		Measurements of each of five items in series a	b	c	d	e	Average for series	Range for series	Other information
1	10–2	8:30	−1	0	−1	−1	−1			
2		9:00	−1	+2	−1	0	0			
3		9:30	−1	0	−1	−1	+3			
4		10:00	+1	+1	+2	0	+1			
5		10:30	0	−1	+3	+1	+1			
6		11:00	−1	+3	+1	0	+4			
7		11:30	0	−2	−1	−2	−3			
8	P.M.	12:00	0	−1	0	−1	−1			
9		1:00	−1	+1	+1	−1	−1			
10		1:30	−3	+1	+2	−1	0			
11		2:00	−2	+1	+1	+3	0			
12		2:30	+2	+1	−2	−1	−5			
13		3:00	+1	+2	+2	+1	0			
14		3:30	0	−1	−2	−2	−1			
15		4:30	−1	−4	+3	+3	+1			
16		5:00	+3	0	+2	0	+1			
17		5:30	+1	−1	+1	0	0			
18		6:00	0	0	−1	−1	−1			
19		6:30	−3	−1	0	−1	−2			
20		7:00	−1	+1	−3	+1	−2			
21		7:30	+4	0	+1	−1	+3			
22		8:00	−1	−2	+1	+1	+1			
23		9:00	+1	+2	+3	−1	−1			
24		9:30	0	−1	+2	0	0			
25		10:00	+3	−2	−1	+2	−3			
Total										
26	10–3	8:30	+1	+1	+3	+3	+2			
27		9:00	−1	+4	+2	+2	+2			
28		9:30	+1	+1	+1	−2	−1			
29		10:00	0	+1	+3	+3	+2			
30		10:30	+2	+4	+4	+3	+4			
Total										
Average										
31	10–4	8:30	+1	−1	−1	−7	+1			
32		9:00	0	0	−2	0	−1			
33		9:30	+3	−3	0	−6	−1			
34		10:00	0	+4	+10	+2	+1			
35		10:30	+3	+4	+4	+3	−3			
Total										
Average										

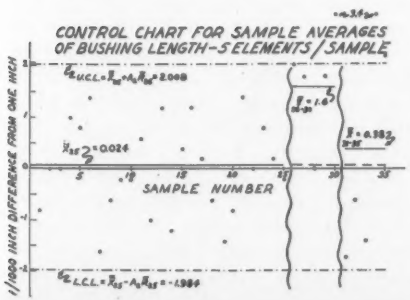

FIGURE 1.

of the samples will behave approximately like a normal distribution, and practically all of the sample means will lie within plus and minus three times the standard deviation of the sample means from the grand mean of all the samples. If all the sample means lie within these limits, called control limits, then we can predict that subsequent sample means will lie within these limits, provided that the cause system does not change. The state of affairs can readily be depicted on a Shewhart control chart (TABLE 1, FIGURE 1). Where a process presents

such a control chart as in samples 1–25 the process is said to be in a state of statistical control. A state of statistical control permits one to predict future behavior within limits, on the basis of past behavior. The state of the statistical control, however, must be attained by the detection and removal of an assignable cause of variation. The control chart for sample means with 3 S.D. (standard deviation) limits is one of the simple criteria for attaining statistical control. The methods of statistical control are necessary (not sufficient) for the economic attainment of quality of manufactured product.

The quality characteristics of mass manufactured products are usually susceptible to accurate measurement by simple techniques. The attainment of statistical control of product quality, however, is often a difficult, drawn-out task. One may look upon production method and production rate as "qualia" of their own respective systems of causes which join in governing the process. Since method and rate are far more variable than the physical characteristics of a product, it is reasonable to expect that it is much more difficult to control and predict these two aspects of mass manufacture than it is to control product quality. An approach to attaining statistical control of the production rate has been formulated by Dr. Adam Abruzzi and the writer.[7] Experimental work has been effectively conducted by Dr. Abruzzi[11] for providing criteria of control in the garment industry. Under grant of the Wertheim Foundation of Harvard University, the writer, together with Mr. Stanley Milberg and Mr. James Thomen, has been investigating operations in the radio, foundry, automotive, and electric cell industries.

In this work, it has been found that the important objective is not to set production rates for the purpose of wage payments, as is the current practice, but to discover and eliminate assignable causes of variation. The existence of assignable causes of variation of production rate gives the clue to changes in production method which can yield more significant increases in productivity than those attained by the mere incentive of increased pay. The attainment of a state of control of the production rate together with product quality is an indication of how far one can go in the maximizing of output. The state of control of these three aspects of production is an ideal called "technological stability." Processes which are technologically stable can be expected to give output in quantity and quality, predictable within limits, and maximum under the prevailing circumstances. Further increase in output for given input requires radical modification of the production method. The criteria of statistical quality control may be expected to provide means for increasing output of processes not yet in a state of statistical stability and for bringing about this state. Technological stability may be considered a state of industrial operation which provides an optimum of effectiveness and which may, thereby, contribute to the overall economic stability of industrial society (see chapter XVI in ref. 9).

The setting of verifiable standards of productivity is a difficult task, employing all the inferential principles of statistical quality control as well as a variety of other statistical techniques. Direct applications of the control chart are made to the time for a complete operative cycle composed of k elements. These control charts must be able to distinguish between the training period and the asymptotic state of stable production. Such chart control must be maintained during the duration of the operation in order to assure that the rate remains in control. It is also necessary to correlate the productive rate established for a given operation with the actual output obtained, for other variables may be effective in modifying the actual output from that predicted. In one company, where some ten plants perform the same assembly operation according to common "standardized" rates, the actual production varies from 6 to 36 per cent less than the expected output. Such rates of production are clearly unrealistic and the production rate cannot be said to be in control.

Among the factors causing deviations from expected output are various "delays." These delays are currently ascribed to personal requirements, fatigue, and to various unavoidable delays owing to technical factors. The meaningfulness of fatigue measurements is questioned[10] and delays are placed in two operationally verifiable classes, avoidable and unavoidable, suggested by Abruzzi.[8] He also studied the statistical control of delays as a means of reducing and stabilizing them. This can be done economically by sampling methods, employing the binomial distribution[12] and the p-control chart. Delay allowances obtained by these techniques are accurate to the extent required. Furthermore, they can be stabilized and maintained. In fact, the stabilization of delay may go a long way toward stabilizing output.

Production rates may be subject to many variations, owing to differences between workers, machines, materials, and conditions. The contribution of these factors to variation in production rates can be studied by the techniques of analysis of variance[13] and statistical design of experiment,[14] and overall variation thereby reduced. A variation that has not been studied adequately by current practitioners is that of the accuracy of these measurements themselves. Not only has no attention been given to the stability of the qualia measured, but the accuracy of measurement has been ignored. By attaining stability of the variables measured, a measure of accuracy can be obtained.

The production operation is composed of elements, the sum of whose individual rates equals the rate of the entire operative cycle. Current practice attempts to determine a "standard" rate for the isolated element, wherever that element is used in any operation. The rates of performance of these elements may be correlated to succeeding or preceding elements and remain inconstant, therefore, for different operations. In fact, it is questionable whether an element has a unique identity, except in a given sequence. If element rates are to be reproducible, they must be statistically independent. The question of the independence of elements was first raised by Gomberg[10] and their possible lack of independence experimentally demonstrated by Abruzzi.[11] In fact, it should have been recognized that the rate of a productive operation is a multivariate chance quantity and that such questions as the independence of elements or the equivalence of the mean rates of operations are subjects for multivariate analysis.[15]

An idea of the theoretical difficulties involved in attempting to standardize productivity rates can be gained from a consideration of the criterion presently available for testing the independence of elements. Suppose that the time of each of k elements of an operative cycle be designated by x_i, $i = 1, 2, \ldots, k$. Then any x_i may be a chance quantity with a distribution function $f(x_i)$. In particular, $f(x)$ may be a normal univariate distribution. If we now consider the set $\{x_i\}$, $(i = 1, 2, \ldots, k)$ together, this set represents a k-variate chance quantity which has a k-variate distribution, related to the individual distributions of each of the k x_i and the correlations existing between them. At present, we can only assume that the set $\{x_i\}$ is distributed as a k-variate normal distribution:

$$f(x_1, x_2, \cdots, x_k) = \frac{\sqrt{A}}{(2\pi)^{k/2}} e^{-1/2 \sum_{i,j=1}^{k} A_{ij}(x_i - a_i)(x_j - a_j)}$$

In this equation

$$A_{ij} = \frac{-\rho_{ij}}{\sigma_i \sigma_j (1 - \rho_{ij}^2)} = A_{ji},$$

$\sigma_i^2 \equiv$ Variance x_i

$\sigma_i \sigma_j \rho_{ij} \equiv$ Covariance (x_i, x_j), $i \neq j$

$$\| A_{ij} \| \equiv \begin{vmatrix} A_{11} & A_{12} & \cdots & A_{1k} \\ A_{21} & \cdot & \cdots & \cdot \\ \cdot & \cdot & \cdots & \cdot \\ \cdot & \cdot & \cdots & \cdot \\ \cdot & \cdot & \cdots & \cdot \\ A_{k1} & \cdot & \cdots & A_{kk} \end{vmatrix}$$

$A =$ Determinant of $\| A_{ij} \|$

For a set of n observations on a k-element operative cycle, the hypothesis that the elements are independent (that is, uncorrelated with one another, assuming normalcy) can be tested by the following statistic:

$$\lambda = \frac{|a_{ij}|^{n/2}}{|a^0_{ij}|^{n/2}}$$

In this equation,

$$a_{ij} = \sum_{\alpha=1}^{n} (x_{i\alpha} - \bar{x}_i)(x_{j\alpha} - \bar{x}_j), \qquad i, j = 1, 2, \cdots, k$$

$$a^0_{ij} = 0 \qquad i \neq j$$

$$= \sum_{\alpha=1}^{n} (x_{i\alpha} - \bar{x}_i)^2, \qquad i = j$$

$$\bar{x}_i = \frac{1}{n} \sum_{\alpha=1}^{n} x_{i\alpha}$$

and $-2 \log \lambda$ approaches the χ^2 distribution with $k(k-1)/2$ degrees of freedom as $n \to \infty$.

This criterion is obviously too complicated for routine use, and a simpler statistic, whose distribution is being studied, is the following:

$$u = \frac{\sum_{i=1}^{k} \sum_{\alpha=1}^{n} x^2_{i\alpha}}{\sum_{\alpha=1}^{n} \left(\sum_{i=1}^{k} x_{i\alpha} \right)^2}$$

The example just considered indicates that the problem of measuring industrial operations requires the sophisticated use of principles of statistical quality control[4, 5, 6] in effecting valid experimental inference,[9] supported by astute use of a wide range of techniques of probability and statistics. Recent experimental work has indicated the possibility of stabilizing the three aspects of industrial operation so as jointly to attain the state of control referred to as "technological stability."

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SECTION OF PSYCHOLOGY

"WHAT IS LEARNED?"—A THEORETICAL BLIND ALLEY*

By HOWARD H. KENDLER†

The frequency with which learning theorists attempt an answer to the question of "what is learned" has led psychologists to invest this question with a conspicuous import. In fact, it is implied by many that this is the crucial question confronting the learning theorist. Other controversial issues presumably stem from divergent conceptions of what is learned. For example, the current controversial issue of latent learning appears to have its roots in the question of whether cognitive maps or stimulus-response associations are learned.

It will be the intent of this paper to analyze the question of what is learned and thereby clarify the issues involved. A methodological analysis can help to separate the question under consideration into its constituent problems. Many theoretically oriented psychologists are prone to forget that scientific method is not governed by the principle of validity at every step of its procedure.[7] During the course of experimentation and theorizing, the individual scientist is forced to make many nonempirical decisions which possess important consequences for the kind of data collected and the type of theory formulated. Included in such decisions are the specific formulations of the empirical questions, the concepts employed, and the way they are defined.

Recognition of the role of volitional decisions in science does not conflict with the conception of scientific objectivity nor does it imply that such decisions should be eliminated. That would be impossible. One of the tasks of a methodological analysis is to make such decisions explicit, so that their influence is readily observable. The importance of the task is testified to by the many examples of progress provided in the history of science by the untangling of facts from the volitional decisions of the scientist.

Our analysis of the question of what is learned will initially attempt to demonstrate that the question does not possess truth character, *i.e.*, it cannot be answered by experimentation alone. Following this, it will be shown how inadequate conceptions of the nature of scientific explanation underlie the disputes surrounding the issue of what is learned.

One may ask, initially, "What is the empirical justification for the numerous contemporary conceptions of what is learned?" Thorndike[8] in 1898, Muenzinger[6] in 1928, Adams[1] in 1929, and Guthrie and Horton[4] in 1946 all report behavior of animals in a problem box situation. It is interesting to note from their observations that they arrive at descriptions of the learning process which appear to be at variance with each other. Thorndike and Guthrie and Horton concluded that stimulus-response associations were learned, although disagreeing to some extent about the conceptual properties of the association. On the other hand, Muenzinger and Adams interpret their data to support the conception that "ideas" or "meanings," concepts which appear to be equivalent to the more modern "cognitive map," were acquired. This difference of conclusion would not be too surprising if disparate findings were obtained. This, however, was not the case. Careful readings of these experimental reports suggest that these experimenters were observing similar behavior patterns of their subjects. Guthrie and Horton write "... Adams' report is so conscientious that we are able to identify in our experiment all the behavior that Adams describes ..." It may be concluded from this that the problem of what is learned, at least in a problem box, appears to be in a class by itself. Similar results are interpreted in such varied and conflicting ways that one is forced to conclude the results actually support none of the interpretations. If comparable data are

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employed in different responses to the question "What is learned?" then we must obviously look to other than empirical sources to understand the conflicting view as to what is learned.

It seems anomalous to offer the structural requirements of a learning theory initially presented by Tolman as guideposts in our methodological analysis. His early papers[9, 10] and the more recent "Cognitive Maps in Animals and Men"[11] stand toward one another as clashing antinomies. Tolman has violated methodological requirements which he himself promulgated. Much of the confusion surrounding the problem of what is learned would dissipate if learning theorists, especially Tolman himself, would be more sensitive and sympathetic to the implications of these early papers. A brief exposition of the structural requirements of a learning theory are, therefore, in order.

Tolman has expounded the pragmatic use of the intervening variable to bridge the gap existing between the independent and dependent variables. Rather than treat separately the relationship each independent variable bears to the many dependent variables, Tolman proposed the grouping of certain analogous functioning independent variables. These groupings or component functions would then be connected logically to constructed intervening variables, and these in turn would be connected to one another and finally to the dependent variables. This intellectual construction has as its aim the economical description of the known empirical relationships, and the prediction of new phenomena.

Tolman's plan for theory construction has instigated both himself, Hull, and others to develop theories of learning. The core of these theories has been the intervening variables, *e.g.*, habit strength, cognitive map, *etc.* Basic to the entire question of what is learned is the specific interpretation given to these intervening variables (theoretical constructs). We shall see that unnecessary elaborations about the meaning of these intervening variables have led to what some have considered to be conflicting views as to what is learned.

There would be no confusion about the meaning of such terms if it were always remembered that these intervening variables serve as economical devices to order experimental variables in relation to the dependent variables. They are "short hand" descriptions, and nothing more, of the "effects" of several independent variables. Because this point has been ignored or misunderstood, an immense amount of confusion concerning the meaning of these intervening variables exists.

This confusion of the meaning of intervening variables is due to the failure to distinguish sharply between the thought processes leading to the theorist's formulation and the operational meaning basis of the term. The first is a problem in the psychology of creative thinking (in this case, the psychology of the psychologist), the second is a problem in epistemology. Why Hull[5] chose to define habit strength (S^HR) in the manner he did is a function of his intellectual abilities, his style of thinking, his personality, his interests, *etc.* In order to know what habit strength (S^HR) is, one need not know the intimate details of Hull's background or have a knowledge of his intellectual functioning; one need only know how this concept is tied down to "observable events," *i.e.*, its operational definition. This point may be likened to the frequent use of models by physicists in their theorizing. These models are useful tools of thought. As Bridgeman[2] has indicated, however, it is very important that the "tool" is not confused with the scientific concept which may stem from it. Too often, learning psychologists have been misled into believing that the intuitive models of their intervening variables possess existential properties above and beyond the operational definition of the intervening variable.

Although psychologists are very willing to admit the existence of individual differences in motor skills, aptitudes, and many other behavior characteristics, they appear markedly reluctant to admit that such dif-

ferences may exist in creative thinking. Would it be valid to say one must think "phenomenologically," as does Tolman, or within a Darwinian framework, as Hull appears to do, or "algebraically," as does Spence, or in terms of physical models, as does Köhler? It is granted that only a valid and extensive knowledge of the psychology of thinking could answer this question. If we refuse to ascribe certain unique and mystical qualities to the process of creative thinking (*i.e.*, we consider it just as another form of behavior), then we may extrapolate from our knowledge of other behavior. We know that people learn mazes in different manners, runners sprint with different styles, and, if we are to believe Kinsey, people woo in different fashions. Cannot creative thinkers think differently? Certainly within our limited (or should we say "nonexistent"?) knowledge of the variables involved in productive thinking, it would appear questionable to assume that only one style of thinking would be fecund; and it would be hazardous, as well as somewhat presumptuous, for any theorist to insist that every other theorist think in his style.

The above point, it should be carefully noted, does not imply that theorizing is purely a matter of personal taste. At some stage in the development of a theory, the theorist must meet the requirements of his scientific audience, *i.e.*, the operational definitions of his theoretical (intellectual) constructs must be made clear to everyone who is sincerely interested in them. This does not mean the theorist must reveal his private cerebration (for purposes of clarity, it may be best if he did not) but that he must state the relations his theoretical constructs bear to the observable variables so that their "validity" may be tested. The only meaning such theoretical constructs possess is contained in their connections to the directly observable independent and dependent variables. The only function they serve is their economical usefulness in integrating the known data and generating theoretical estimates of behavior. These intervening variables are essentially imaginary concepts and to concern oneself with the problem of what they really are or what properties they really possess appears to this writer to be a pseudo-problem. The most we can expect from a learning theory is that it generate theoretical estimates of the dependent variable in agreement with the observed responses in an experimental situation. All learning theories must fulfill this function, *i.e.*, generate estimates of the to-be-observed response. In this sense, all learning theories are response theories.

The above analysis leads this writer to conclude that Tolman's recent division between place learning and response learning is not sufficiently cogent. It suggests a qualitative distinction where none exists. This is not equivalent to stating that differences between so-called "response" and "cognitive" theories do not exist. There are some very crucial differences between certain S-R and certain cognitive theories, *e.g.*, differences in implied conceptions of scientific explanation, in the precision of theoretical formulations, in techniques of introducing and defining theoretical constructs, and in the interpretation of certain experimental variables. The important point is that such differences do not stem directly from an S-R formulation as contrasted to a cognitive formulation. The validity of this last point can be attested to by the fact that the pattern of theory construction utilized by Hull, an S-R theorist, has been identical to the type espoused by Tolman, a cognitive theorist. There is no general agreement, moreover, among the S-R theorists or among the cognitive theorists concerning the proper position to be taken on the aforementioned "crucial" problems.

The pseudo-problem of what is learned has arisen because of the contamination of the operational meaning of theoretical constructs by their intuitive properties. This contamination has led to spurious formulations of theoretical differences. Rather than persistently pursue both the obvious and subtle connotative meanings of the

words used in theorizing, it would be exceedingly more productive to attempt to relate the constructs systematically to the manipulable variables and behavior measures.

Why have learning theorists spent so much time in describing the conceptual properties of their theoretical constructs at the expense of determining their relationships to the observable variables? It is this writer's belief that the answer to this question lies in different conceptions of the nature of scientific explanation.

Feigl,[3] in a brief perspicuous exposition, states that the demand for explanation "is answered by deductions either from empirical laws or from theories." This requirement for a deductive component (whether it stems from an inductive-deductive component or hypothetico-deductive approach, is unimportant for the present discussion) has too often been ignored in scientific explanation. This writer believes that the reason for ignoring the necessity of a deductive component in scientific explanation stems from the failure of some to distinguish sharply the requirements of scientific explanation from those attributes of propositions which instigate for some a feeling of understanding (the "a-ha" phenomenon or "psychological closure"). It is the contention of this paper that some of the arguments concerning what is learned have stemmed from this confusion between scientific explanation and what may be called psychological understanding. This confusion has been particularly noticeable in some formulations involving physiological and/or phenomenological terms.

A common misconception among some psychologists concerned with discovering the physiological correlates of behavior is that their area of interest excuses them from the task of developing a theoretical system capable of generating valid deductions. Believing that they are dealing with the "real causes" of behavior, they feel that the mere specification of the physiological factors or, more commonly, the mere speculation concerning the physiological processes is sufficiently explanatory. Adequate explanatory systems in physiological psychology, as well as in other areas of psychology, must have a deductive component.

The above should not be interpreted as an attempt to make an invidious comparison between behavioral and physiological theories. The development of psychology can and should be furthered by serious and rigorous attempts at formulations of both behavioral and physiological theories. This writer believes (on an intuitive basis) that, at the present time, the program of independent development at both levels would be the most strategic, followed, of course, by an attempt at coordination. Another procedure would be to develop behavioral and physiological formulations interdependently and simultaneously. There is no reason why such attempts could not be successful. The tendency, however, has been merely to use some physiological-sounding terms, extracted in all liklihood from the private thought processes of the theorist, without specifying their relations (hypothetical or established) to the experimental variables and behavior measures. Although such a procedure provides a ready answer to the question under consideration (what is learned?), these answers contribute nothing to our understanding of learning.

A similar problem is raised by some theories which use phenomenological terms as labels for their theoretical constructs. It appears that for many learning psychologists it is essential that their theoretical constructs be phenomenologically consistent, *i.e.*, capture the quality of naive introspective experience. There is little doubt that the theoretical constructs used by psychological field theorists reflect "inner experience" in a more satisfying manner than do associationistic constructs. There is no basic methodological objection (or virtue) to the use of either set of terms as long as they are related to observables, and as long as they are used in formulations with deductive capacities. This rule, however, is not always followed. All too frequently, constructs by themselves (*e.g.*, structuring, insight), which describe vividly phenomenological experience, are presented as scientific explanation.

It is this writer's conviction that the

selection of any theoretical model, be it physiological, phenomenological, physical mechanical, or mathematical, is, in the last analysis, a decision. We should attempt, in a sincere and conscientious manner, to understand the implications of such decisions, but we should not be led astray by believing we can demonstrate their validity.

What then should the learning theorist do? It is this writer's opinion that rather than seek an answer to the question of what is learned, in a manner based upon false conceptions of scientific meaning and scientific explanation, it would be better for learning theorists to come to grips with their undertaking in a positive forthright fashion, *i.e.*, to relate their theoretical concepts to observables and unhesitatingly test the explanatory capacity of their formulations. By so doing, the time wasted in needless arguments of the sort which has centered around the question of what is learned would be reduced and, perhaps, even be eliminated.

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*SECTION OF ANTHROPOLOGY**

LAND USE IN THE RAMAH NAVAHO AREA, NEW MEXICO†

By JOHN L. LANDGRAF‡

In much ethnographical writing, there seems to be a tendency to reify traditionally derived academic "culture" (or "society") abstractions, and to allow them to limit or distort the descriptive process. It appears that what has happened is that the anthropologist, out of the developing traditions of his field has frequently tried to see in the actual field situation only that part that he has been told to look for (a particular "culture"), and, as a result, only that part has appeared in his work, continuing what has sometimes been an original error. This process of continuing reification can be seen in earlier studies in the "classical" period of anthropology, as it can also be seen in the later "acculturation" studies. It still exists, although there is a growing tendency, perhaps born out of demands for practically usable descriptions, for redefinition of terms, and for the use of wider frames of reference.

The long list of "cultures" which the anthropology student learns and works with has not been derived completely by the same principles, explicit or implicit, and a portion has not been derived at all, only accepted as fact, whether it came from travelers' tales or from one or another outgroups to the people being studied. In addition, there are many continuing separate organizations of life which have never been dignified by a place in the traditional list, although we have often implicitly recog-

* The Division of Mycology held a meeting on November 24 at which a paper entitled "Newer Techniques for the Study and Diagnosis of Fungus Diseases, with a Critical Evaluation of Methods Used in the Therapy of Superficial Fungus Infections," was presented by Doctors Donald M. Pillsbury and Albert M. Kligman. An abstract of this paper will appear in a forthcoming issue of TRANSACTIONS.

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nized them and have often accepted their reality in field work.

The actual process which we apparently see before us is the constant modification of cultures into "social categories" or "status groups" in larger cultural frameworks, and to some degree, the reverse of this process. It is a handicap to be required to speak of the "culture" of a family and the "culture" of Western Civilization, necessary as we find the common concept to be.

In general, this study points out that "contact" situations are such integral parts of any of the contemporary social units which the anthropologist studies that, in order to describe behavior realistically, a constant awareness must be had of a widely expanded hierarchy of cultural "orders." It is not possible to confine oneself to the Hopi, as Laura Thompson[1] and Dorothy Eggan[2] have recently pointed out, or to the Navaho, or to any others of the traditional anthropological list. We must always include as part of the picture the various other configurations that fall within the broader one of western civilization. This is not so much "acculturation" as it is straight anthropology with a more realistic frame of reference.

The present study began as an attempt to describe the relations of a small, relatively isolated group of Navahos with their geographical environment. The basic methodology was anthropological, with an attempt to satisfy geographical criteria as well. The concept of land-use is a geographical one, referring mainly to the "material manifestations" on the land surface, but we know in anthropology that such a concept must be redefined in functional, historical, and even psychological terms. "Land-use" is only an arbitrarily defined aspect of a whole culture and geographical environment, at a particular time, modified by other cultural contacts, and relatively meaningless without such a frame of reference.

The material here was gathered during a period of seven months in the field, in 1941 and 1949, together with much work in the field notes of others who have worked in the same locality. Clyde Kluckhohn of Harvard and a number of his associates and students have been working since about 1936 in the country around the little Mormon village of Ramah, a few miles east of the Zuni Indian Reservation in west central New Mexico. Until the last few years, the work was directed toward the local Navahos. The materials from the various studies were gathered into a master file at the Peabody Museum at Harvard, and a number of published articles have described aspects of the work.[3] At the present time, an expanded study, under the sponsorship of the Laboratory of Social Relations at Harvard, is is going forward. This study, of value orientations in differing cultural groups, includes various Anglo-Saxon groups, another Indian one, and the Spanish-Americans. It covers a larger geographical area than does the present work. It is an example of recognition of the point of this paper.

The Ramah Navaho area is defined largely by the specially-oriented activities of the local Navahos. It comprises a little over fourteen townships, about five hundred square miles, located, as mentioned, just on the eastern edge of the Zuni Indian Reservation, with the Zuni Mountains rising out of its northeastern quarter. The land is high, over 7000 feet in elevation, with a number of ridges or "mesas" crossing it, interspersed with flat drainages and smaller valleys or canyons. The southeastern fifth of the surface is covered with basaltic lava flow, and two drainages run out of the area to the southwest and west. The latter waterway is a part of the Zuni River and both drainages form the very headwaters of the Little Colorado River. The Continental Divide is only a few miles east of the Ramah area.

The underlying strata are composed mostly of sandstones and shales, and they frequently form outcrops where they have not been covered with lava. The soils are thin, undeveloped, and contain large amounts of "alkali," particularly in the drainage-ways. The drainages are of the semi-arid type, ephemeral in character.

Ground water is not abundant and surface water supplies quickly disappear through evaporation or percolation. Annual precipitation, although in the semi-arid category, is highly variable in time and immediate locality. The annual averages range from 6.71 inches to 23 inches, the monthly averages from .45 to 2.30. There are roughly four annual climatic periods, with wet seasons in the winter (snow) and summer (rain). Precipitation is the major determinant of plant growth and the summer rain is the major determinant of agricultural success. The growing season is short.

The wild vegetation falls in the category called "piñon-juniper," and these trees, together with some yellow pines, occur mostly on the ridges. The flat drainage-ways are generally covered with perennial composite shrubs, although one district seems to have been invaded by sage. The chief livestock forage plant is black grama, which appears to be losing ground against grazing uses by sheep and cattle. The areas of woody perennial shrubs seem to be gaining at the expense of the trees and grass. Areas of intensive human use are usually covered by annual "weeds," Russian thistle ("tumbleweed"), and sunflowers.

Intensive livestock grazing and the many abandoned farm areas and roads, covered only at certain seasons of the year with annual weeds have contributed to the development of recently intensified water erosion processes, which have cut deep gullies or arroyos during the past thirty years. Wind erosion also seems to have taken its toll in some localities where large-scale dry-land farming has been undertaken. In general, the Ramah Navaho area does not seem suited to any but the most simple and small-scale farming enterprises, and its grazing resources, while perhaps large in earlier times, have been progressively destroyed by over-use of the land. The cultural uses of the land do not seem to have included many preventive techniques for the destructive processes at work, and these uses have hurried along this destruction in every way. There seem to be very few other potential uses for the area. Since the thirties, there have been some Federal government erosion control measures, but these have been insufficient in the face of the constantly increasing intensity of grazing and farming activities.

In 1941, the Ramah area was connected by graded dirt roads with the nearby trading centers of Gallup and Grants (both about 45 miles away) and with Zuni (20 miles) and Atarque, a small Spanish-American village to the south (7 miles). Within the Ramah area, there were many smaller two-rut tracks through the trees and shrubs, so that nearly all of the territory could be covered by wagon or small truck in dry weather. Horse trails were everywhere. The largest trading center and population concentration was Ramah village, in the northwestern corner of the defined area. Elsewhere, there were several other trading stores, a tiny Spanish-American village (Tinaja, about twelve miles east of Ramah), a Federal government reservation (the Inscription Rock National Monument), and an emergency landing field under the Civilian Aeronautics Administration. A radio and weather station was maintained here. There were dwellings scattered thinly over most of the area, and cultivated fields scattered over all but the lava plains in the southeastern quarter. Most of the fields were located in the northern half, on the slopes of the Zuni Mountains, or in the narrow irrigated valley at Ramah Village, supplied by an earthen dam there.

In 1941, the Ramah area was occupied by about one thousand people. About half of these were defined as Navahos. The rest ("whites"), included about 40 Spanish-Americans ("Mexicans" to some of the local people). The other whites, generically called "Anglos" in New Mexico, included about 300 Mormons in Ramah Village, about 120 "Texas people," of which more later, and about thirty people of diversified midwestern American background, called "outsiders" in this study.

In 1941, the Ramah Navahos were scattered irregularly over most of the area.

Their dwellings were usually small clusters of rough hogans, with a few crude log cabins and other structures in some of the more permanent locations. There were no privies, and household water was hauled in barrels. The small cultivated fields, largely of corn and beans, were located at the heads of valleys or less often at their mouths. Much use was made of plows and some other items of white machinery, although the crop yields were not so great as those of the local white farmers. In 1941, there were some 3000 acres in Indian crops. Perhaps less than a third of the local Navaho subsistence came from this production.

Although the people had comparatively large numbers of horses and a few cattle, their chief livestock activity was with sheep (and a few goats). These were all grazed on open ranges the year around. Sheep shearing was done in the late spring and lambs were sold in the early fall. These productions, as well as pelts and woven blankets, were traded to the local trading stores, with whom the Indians maintained annual credit relations. As was the case with farming activities, nearly all of the Ramah Navahos learned basic skills with sheep and horses. For the approximately 21,000 sheep units of stock that they owned, however, the Indians had land available to them in 1941 which government grazing men calculated would safely carry only about 6400 units. Their land was thus heavily over-grazed, and in addition, when they could, they used land that was not legally theirs. Perhaps a little more than a third of the group income came from their livestock, principally from their sheep. This was the economic activity of most importance in the Navaho value system.

Other than the small increment from hunting or other uses of wild products, and home manufactures, the rest of the Navaho income came from wage-work, mainly for white farmers and livestock operators. All of the work was in the unskilled category, and the pay was low. In 1941, the Texas people who had large farms in the northeastern part of the Ramah Area depended considerably on the local Navahos for their field labor, and various single Navahos worked for those of other groups as well. A few even worked outside of the area. There was no public works program on Indian land, as there had been only a few years before. Federal government assistance was relatively small and indirect, although the administrative controls that had been set up during the earlier New Deal period were still in formal operation.

Our direct knowledge of the Ramah Navahos begins shortly after 1870, a few years after the whole Navaho tribe had returned from the four-year period of confinement by the Army at Fort Sumner, in eastern New Mexico. About ten families returned from the captivity to begin life anew in their old territory. They had a direct contact with at least a part of Anglo-American culture to add to the previous long history of interaction with various Pueblo peoples and the Spanish-Americans on the Rio Grande. They were very poor in those first years but we know that they had knowledge of a great many out-group materials and techniques. We know that sheep-culture, weaving, silver work, and even agriculture occupied less of the time then, and more time was spent at hunting and seed-gathering.

Agriculture was somewhat along the lines described by Hill[4] and the animal husbandry practices of the time were exceedingly simple. The small number of sheep and goats (about one hundred for the fifty persons in the band) were probably those given out by the Federal government at the time of the release from captivity. Lambs were not sold commercially nor was wool clipped for other than local domestic needs. There were certainly horses and dogs, and probably no cattle. The arrangement of land-use was in the pattern called "inherited use-ownership" by Hill, and water was considered to be open to all who had use of it. Movement over the landscape was undoubtedly more fluid than in later years, although the people were not "nomads."

The arrangement of interpersonal rela-

tions within the group was almost completely determined by kinship, and interactions within this framework must have displayed a stability that is not true today. The religious or ritual patterns were integral parts of the whole culture, active in every detail of individual behavior and organized into a relatively systematic framework.

In general, it can be theorized, as Kluckhohn[5] has done, that the Navahos of the time were in a psychologically confused state of mind in their relations with outgroups. The four years of captivity at Fort Sumner must have given them a deep and frustrating experience of the military and economic power of the Anglo-Americans. The classical Navaho war and "enemy" patterns had been forcibly suppressed. This suppression apparently also shadowed relations with other outgroups than the Anglo-American. When the Ramah Navahos returned from Fort Sumner, they found that Spanish-American sheep herders from the frontier villages of the Rio Grande towns had begun to settle in their territory. There was much open country, however, and the land-use patterns of the newcomers were enough in accord with those of the Navahos to make the situation acceptable. The Navahos stayed in the northwestern part of the area we have defined here, and the Spanish-Americans moved around to the eastward, expanding toward the southwestern plains, where Atarque now stands.

There was much contact, however, for Navaho men were given Spanish names such as Cojo, Martine, Garcia, and others, names which they have continued to use in their relationships with whites. Many of them learned some Spanish as well. From the Spanish-speaking people, the Navahos undoubtedly got additional sheep and goats and vastly improved methods of husbandry. Various patterned relationships were set up, in which the Spanish-Americans used the Navahos as sheep-herders, and as farm-hands on their small irrigated fields. In a sense, the Indians came to be thought of as parts of the feudal holdings of the landlord *patron* figure who became established at Tinaja. Later, a similar organization developed at Atarque, near the southern part of the Ramah Area. In many ways, the Spanish-Americans, the dominant group of the period, seem to have respected the cultural integrity and personal dignity of the Ramah Navahos.

The Anglos began to move into the region only a few years after 1870. Some, cattlemen and traders, came from the east, from the Rio Grande and from Texas. Others, Mormons, came from the west, from the expanding or changing Mormon colonies in Arizona. After exploration of the region in the seventies, the Mormons founded the irrigated settlement of Ramah (then called Navajo) in 1882. The town was built in the midst of the Navaho range of the time, and, in spite of this, the Indians do not seem to have shown any explicit reaction to the invasion. Implicitly, however, they seem to have developed a kind of passive animus, still to be seen in 1941, toward the somewhat patronizing friendliness of the religiously inspired Mormons. With only superficial exceptions, the Mormon attempts to missionize the Indians failed and only a few of them would work for, or learn from, the settlers. Toward "their Navahos," on the other hand, the Mormons seem to have developed a feeling of parental proprietorship, which became almost a simple economic exploitation. Trade with the Indians and use of their lands and labor were easy sources of additional income for the Mormon settlers. They apparently never recognized the reaction on the part of their outwardly peaceful neighbors.

Early in the eighties, the general region was surveyed and the Homestead laws began to operate. Checkerboarded sections were awarded to the railroad that was pushing its way across the country to the north. Only gradually, however, did legal land lines begin to be functionally established, and there was much use of open or "free" grazing land. Conflicts of interest were settled by immediate force. Thus, with the Mormons and with Anglo cattlemen, the Ramah Navahos began to feel the whole contact

situation. Not only did they meet differences of land use and property, but they also met contemptuous "racial" attitudes that gave them no personal or cultural respect.

After the land restrictions, the second most important effect of the coming of the Anglos was the establishment of trading stores. For what must have seemed to be simple efforts on their part the Indians got what they must have considered to be extremely valuable goods, manufactured foods, trinkets, and metalwares. It apparently did not matter at the time that the prices charged were amazingly high. As the Mormons said, "a man could make his stake in a year in those days." As the trade developed, the Navahos learned to use and depend on many of the white man's artifacts, and their own productive activities came to be directed more toward products acceptable to the traders. Sheep culture came to be emphasized over other activities. The kind of annual credit arrangements that have been described came easily into being. Understanding of the workings of the system were very slow to come to the Navahos, however.

In the first two decades of this century, other Anglos than the Mormons began to have direct contact with the Ramah Navahos, for Texas cattlemen began to expand their activities over the whole region, and there was little place for either Navaho or Spanish-American sheep men. It was at this point that the most successful local trader, a non-Mormon Anglo, persuaded the Federal government to intervene on the side of the Indians, who had legal right over virtually no land in "their" territory. With the authority of the Dawes Act of 1877, allotments of a quarter section to each Navaho were made from available public domain. The scattered land which was available, however, was largely in the poor southern districts of the Ramah Area, much of it on the lava plains. It was ludicrous to suppose that a quarter-section of such land, or even a section of it, would support a single Navaho. It was allotted, however, and the Indians began to move to their allotments and to use whatever additional land they could.

In the meantime, the Spanish-Americans had begun to lose out in their battle to maintain their dominant position. Their settlements at Tinaja and at Atarque to the south of the Ramah Area began a decay from which they have never recovered. An absentee Spanish-American landlord took over the bulk of the Tinaja district holdings and "outsiders" took over control of the Atarque ranges. The Texan cattle ranches, too, began to lose their position in the face of post-war price falls as the twenties began.

In addition, in the early twenties, a new group began to come into the Ramah area, primarily into the Tinaja district. These people were former sharecroppers from Texas, who came west to "pioneer" on homesteaded land. They brought with them large-scale dry-land farming techniques and they cleared and plowed land for pinto beans, a relatively successful crop in the general region. Their settlement grew into a definite community in and amongst the Spanish-Americans around Tinaja.

The thirties brought many difficulties, some of which reflected the growing importance of the "outside" in Ramah Area affairs. A disastrous snowfall in 1931 killed thousands of head of Navaho and white livestock and this only accentuated the general economic hardship among the various farmers and stockmen. A new wave of Texas dry-land farmers arrived to settle the remaining public domain in the southern part of the Ramah Area, and the Mormon irrigated fields at Ramah began to lose their productivity, due to lack of drainage in the fields and resulting accumulations of mineral salts. Expansion problems, too, had created a group of young men there, trained to the community goals and unable to obtain them. Much of the resultant community aggression seems to have been taken out on the Navahos.

This, like the years of the first World War, was a particularly trying period for the Navahos. As in the previous period, they appear to have followed modes of expression partly culturally allowed and partly newly borrowed. Ceremonial participation and witchcraft accusations, for instance, seem to have increased, as did drunkenness and the release it gave for culturally disallowed intra-group direct hostility. No overt expression could be directed toward the whites or other "enemies," and as a result the aggression was turned inward.

What can be seen by this time is a series of established communities ("cultures") in the area, each with its own integral stake in the land, and the whole overstraining the resources of that land far beyond its capacity. All of these groups had been modified by the sequence of inward migration and land change, and yet all of them maintained their psychological cultural identity and their value systems, which came from generalized background cultures. On top of this, the outside world increasingly imposed goals, controls, and barriers which were not understood nor particularly subject to manipulation.

This state of affairs was brought sharply into focus by the entrance of the Federal government into the area with the "New Deal." Primarily, the government entered the area on behalf of the Indians, and the Indian Service began an intensive program of helping the Navahos at the expense of the local whites. "Free" land was leased from its legal owners for Indian grazing use, and strong protection was given to the Indian against unfair "deals" and trespass habits, which had grown to be virtual "customs" of the area. To the Indians, however, the "assistance" was confusing and even threatening, especially when it included controls over livestock activities and other admonitions that violated basic Navaho assumptions. The men who drafted the programs and those who administered them in the field were "outsiders," and they began to provide the definition of a new group and culture in the Ramah area (although there had long been some representatives of the "outside" there).

In 1941, as the national defense program grew, the Federal government was beginning to withdraw. The whites, especially the Mormons, were beginning to reassert themselves with a vigor born partly of aggressions that had not been allowed expression against the Federal "outsiders." The Navahos were not far enough along any road of cultural modification to compete successfully without subsidy, although they had been stirred to further insecurity from their explicit passive adjustment to local white (and especially Mormon) overlordship.

The period since 1941, although not well documented here, has seen modification of the Ramah Mormon culture to more close adjustment toward the local terrain and the "outside" culture, with concomitant movement away from theocratic values and irrigation farming goals (which are now represented by virtually unused fields and irrigation works, maintained as symbols of old separate cultural unity). Amongst the Navahos, there has been further retrenchment in the face of Mormons, although, during the war years, there was a vigorous attempt at community self help. A government day school now provides a new community focus on a new level, while an evangelical mission establishment symbolizes further disruption (and little integration on a new level) of the old cultural orientation. Although data are lacking, it would appear that the grazing range has degenerated further and that increased population and a smaller land base has further constricted the Navahos economically. The Spanish-Americans are virtually gone and the "outsiders" are much less in evidence. The Texas dry-land farmers are still active, although their land has suffered perhaps more than any other from erosive destruction.

This description has presented in extremely compact form the dynamics of a particular anthropological "culture" when it is seen in a larger framework than that generally used in many of the studies we

know. In this space, it has not been possible to deal with many of the pertinent geographical, economic, sociological, and psychological ramifications that are all parts of the immediate configuration that exists amongst the Ramah Navahos and their use of the Ramah Navaho area. What we actually have now in the area is almost a "social structure," instead of a series of established communities or cultures, and indeed, from many points of view, we have actually only a single status group in a very much larger social structure. As was mentioned at the beginning of this paper, there is need for concepts which will enable us to keep our bearings in all levels of abstraction which the growth of western civilization has imposed on the reality behind our list of anthropological "cultures." Even without these needed concepts, there is need for careful use definition of our present terms, so that they can be exactly placed in larger or different configurations by others.

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SECTION OF OCEANOGRAPHY AND METEOROLOGY

SOME COMMON CHARACTERISTICS OF THE GULF STREAM AND THE ATMOSPHERIC JET STREAM*

By COLUMBUS O'D. ISELIN†

Between Cape Hatteras and the Grand Banks of Newfoundland, the Gulf Stream, on the average, follows the great circle course. This not only takes it well away from the continental slope, but also, for the first time since leaving the Straits of Florida, the current finds itself in really deep water. South of Cape Hatteras, the current is flowing over the Blake Plateau in depths of 1,000 meters or less. At least as far as the longitude of Halifax, depths of four or five times as much prevail. South of Halifax, a number of isolated sea mounts, which rise to within 1,000 to 1,500 meters of the surface, have recently been discovered, but it is almost certain that no obstructions occur between this point and Cape Hatteras.

The reaction of the current to the sudden freedom it experiences on leaving Cape Hatteras is to take on an increasingly meandering course. This was only discovered a few years ago when the establishment of a Loran navigational network off this coast made it practical for a research vessel to follow the inshore edge of the swiftly moving water. Loran navigation also made it possible to gain a good measure of the width of the current, as well as its strength, and it was found to be much narrower and swifter than had previously been supposed. It is interesting to note that much of our understanding of the Gulf Stream has been derived through improvements in navigation.

Theoretical studies by Stommel (1948) and Munk (1950) have shown that a narrowing of the stream lines is to be expected in the western part of each ocean as the result of the variation of the Coriolis force with latitude. More recently, Haurwitz and Panofsky (1950) have shown that a wave-like path is also to be expected when the current is not confined by the continental slope. Nevertheless, it cannot be claimed that the available theory is adequate at present to explain in satisfactory

* This paper was presented at the meeting of the Section on November 28, 1950.
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detail many of the observed characteristics of the Gulf Stream.

Until very recently, the field observations were limited to what could be obtained by a single ship. The techniques used during the last four or five years in Gulf Stream surveys have been described by Iselin and Fuglister (1948). While these were a great improvement over the pre-war methods, the area that can be covered by a single ship in a reasonable length of time falls far short of the scale of some of the Gulf Stream phenomena. During June of this year it became possible for the first time to carry out a multi-ship survey of the Gulf Stream between Cape Hatteras and the Grand Banks. This became known as operation CABOT.

The survey was conducted during the period June 6–22 under the direction of Doctor Richard Fleming, Chief of the Division of Oceanography of the Hydrographic Office, which supplied two of the vessels: the *San Pablo*, serving as the Headquarters Vessel, and the *Rehoboth*. The Woods Hole Oceanographic Institution also supplied two much smaller vessels: the *Atlantis* and the *Caryn*. The U. S. Fish and Wild Life Service furnished the *Albatross III* and the Naval Research Establishment, Halifax, the *New Liskeard*. All told, about 40 oceanographers took part in the operation.

On the *San Pablo*, daily plots were made of the information coming in by radio from the other vessels and from a plane which operated out of Norfolk. The plane was surprisingly successful in spotting the inshore limits of the current during favorable weather. In turn, frequent messages went out from the *San Pablo* advising the other vessels how best to proceed. In this way, the course of the current was traced along its whole distance from Cape Hatteras to the Grand Banks during the first week of the operation.

During the second week, most of the vessels were employed in exploring in detail the large southward loop which had been located south of Halifax. As the vessels returned to the westward, they relocated part of the path of the current, roughly two weeks after the start of the survey.

Although the extensive data collected are still in the process of being worked up by Mr. F. C. Fuglister of the Woods Hole Oceanographic Institution, the diagrams which he has so far completed, when combined with previous findings, permit some general conclusions. In addition, as has been pointed out by Professor C-G. Rossly, it has become fairly evident that the Gulf Stream has several features in common with the jet streams of the upper atmosphere. Thus, we seem to be dealing with rather general phenomena, characteristic of the circulation within a thin envelope on a rotating sphere. In both cases, situations arise where the stream remains concentrated for much greater distances than our present understanding of lateral frictional forces can explain. There seems to be a preferred width and a preferred velocity to the currents, the necessary energy having been acquired well upstream from the point when the jet begins to develop. In both cases, perhaps, physical obstructions are the cause of the development of extreme meanders. In any case, the cause seems to be located well down stream from the area where the meanders first begin to form. As they grow, the waves pile up on each other so that strong north-south currents can be encountered where the flow is from west to east on a statistical basis. Finally, the loops may break off leaving clockwise eddies north of the mean track and anti-clockwise eddies to the south.

In the case of the Gulf Stream, these eddies are characteristically 100 to 200 miles in diameter. The distance between the crests of the waves, where they first begin to develop in the sector south of New England, is also about 200 miles.

Roughly speaking, the sorts of changes that take place in one day in the atmospheric jets require a week or more in the Gulf Stream. Also, the dimensions of the system are considerably reduced in the ocean. Thus, even a relatively slow ship

is sometimes able to follow the development of an individual feature, such as an eddy.

Through a new system of measuring currents from a ship under way (VonArx 1950), as well as from the improvements in navigation due to Loran, it has become evident that the cross-current profile of surface velocity is not symmetrical. The maximum surface velocities lie close to the left hand or inshore edge of the stream. Thus, there is considerable thought being directed among oceanographers at present to the best means of defining the limits of the Gulf Stream. As a practical matter, it has been found that if the bathythermograph is used diligently, the point at which the 65°F isotherm descends to 300 feet usually coincides with the sudden increase in surface velocity which is experienced by a ship approaching the current from the coast. This is the definition employed on operation CABOT. In working up the data, Mr. Fuglister has shown that there are certain advantages to be gained by plotting the average temperature of the water column down to a depth of 200 meters, in that this method also indicates the offshore limits of the current. Definitions based on absolute values of the surface velocity are less satisfactory because these cannot be derived until after 20 or 30 minutes have elapsed by the methods presently employed and the ship has consequently progressed another three miles or more. Also, shallow, small, frictionally driven eddies form along the edges of the current.

In such respects, the investigation of the details along the margins of the Gulf Stream is well ahead of knowledge of corresponding phenomena in the atmospheric jets. Not only is the navigational problem simplified at sea, but also it is easier to obtain closely-spaced temperature soundings.

Finally, it was possible, by assembling all six ships of operation CABOT during part of the survey period, to observe in detail what went on during the birth of a large anti-clockwise eddy which detached itself from the Gulf Stream south of Halifax after the second week of the operation. This is the equivalent of the formation of a cyclone in the westerlies at high altitudes.

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CONFERENCE HELD

SECTION OF BIOLOGY

"LEPROSY"

NOVEMBER 10 AND 11, 1950

The Section of Biology held a Conference on "Leprosy." Doctor Emanuel Grunberg, Hoffmann-La Roche, Inc., Nutley, N. J., was the Conference Chairman in charge of the meeting.

The program consisted of the following papers:

Friday, November 10

Morning Session. Chairman, James A. Doull, Leonard Wood Memorial, Washington, D. C.

"History of Leprosy in the United States," L. F. Badger, U. S. Public Health Service, Atlanta, Ga.

"The Bacteriology of Leprosy," John H. Hanks, Leonard Wood Memorial, Harvard Medical School, Boston, Mass.

"The Epidemiology of Leprosy," Fred C. Kluth, Leonard Wood Memorial, Washington, D. C.

"The Pathology and Pathogenesis of Leprosy," George L. Fite, U. S. Public Health Service, Bethesda, Md.

Afternoon Session. Chairman, Charles M. Carpenter, University of California School of Medicine, Los Angeles, Calif.

"The Laboratory Diagnosis of Leprosy," Malcolm H. Soule, University of Michigan, Ann Arbor, Mich.

"The Universal Serologic Reaction in Leprosy" (two papers), Reuben L. Kahn, University of Michigan, Ann Arbor, Mich., and J. Thiers Pinto and Arnaldo Zeo, Clinic of Dermatology and Syphilology, Faculty of Medicine, University of Brazil and Leprosarium, Curupaiti, Rio de Janeiro, Brazil.

"Lepromin—Mitsuda's Reaction—With Experimental Observation in Dogs," William H. Feldman, A. G. Karlson, and J. H. Grindlay, Mayo Foundation, Rochester, Minn.

"The Detection of Leprosy by the Dermatologist," Frederick Reiss, New York University Post-Graduate Medical School, New York, N. Y.

"An Interpretation of the Ocular Manifestations of Leprosy," David C. Elliott, Veterans Administration, Harrisburg, Pa.

Saturday, November 11

Morning Session. Chairman, George L. Fite, U. S. Public Health Service, Bethesda, Md.

"Murine Leprosy—Its Usefulness as an Experimental Infection," Charles M. Carpenter, University of California School of Medicine, Los Angeles, Calif.

"Chemotherapy of Murine Leprosy," E. Grunberg and R. J. Schnitzer, Department of Chemotherapy, Hoffmann-La Roche, Inc., Nutley, N. J.

"The Treatment of Leprosy," Paul T. Erickson, U. S. Public Health Service, Lexington, Ky.

"The Social Stigma of Leprosy," Eugene R. Kellersberger, American Leprosy Missions, Inc., New York, N. Y.

"The Control of Leprosy," James A. Doull, Leonard Wood Memorial, Washington, D. C.

The Section of Biology provides conferences for active workers in the special fields of Biology.

Attendance is limited to those invited to participate in these conferences and to interested Members of the Academy.

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